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***Comparing Direct and Indirect Forms of
Intergroup Contact in Cyprus***

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ABSTRACT

This thesis examines and compares the effectiveness of direct and indirect types of contact in leading to short- or longer-term prejudice-reducing outcomes in Cyprus. Chapter 1 provides a background to the relations between Greek and Turkish Cypriots and Chapter 2 provides a theoretical introduction to the intergroup contact hypothesis (Allport, 1954) and to extended friendships (Wright, Aron, McLaughlin-Volpe, & Ropp, 1997), vicarious contact (Mazziotta, Mummendey, & Wright, 2011), and imagined contact (Crisp & Turner, 2009) which have been suggested to be alternatives and a stepping stone to direct contact when the latter is absent. Chapter 3 consists of three experiments assessing the relative effects of direct and vicarious contact (Experiments 1 and 2) and imagined contact (Experiment 3). The results show that direct, and to a weaker extent, vicarious contact lead to more positive outgroup attitudes, but that a week after contact this effect is lost. All types of contact yield less anxiety, an effect that endures in time, and direct and imagined contact yield more positive action tendencies, an effect that remains significant in time only for direct contact. Chapter 4 consists of two experiments further exploring the capacity of imagined contact to yield positive intergroup outcomes. Experiment 4 tests whether the induction of interpersonal and intergroup similarities and/or differences into a positive imagined contact scenario affects participants evaluation of the outgroup. The results show, in line with the Optimal Distinctiveness Theory (Brewer, 1971), that ‘balanced similarity’ which incorporates both similarities and differences yields more positive outgroup attitudes than the conditions focusing only on similarities or only on differences. Experiment 5 compares ‘balanced similarity’ with positive imagined contact and finds that only the former affects variables related to preparing individuals for future contact. Chapter 5 consists of a three-wave longitudinal study examining the temporal effects of direct and extended friendships on outgroup attitudes and their mediation. Both types of friendships yield a significant indirect effect on attitudes which is stronger for direct friendships and is mediated by intergroup anxiety for both types of friendships and also by ingroup norms for direct friendships. Chapter 6 presents and discusses the key findings, outlines the limitations of these studies, and suggests avenues for future research.

EXTENDED ABSTRACT

There is now compelling support for Allport's (1954) intergroup contact hypothesis and its effectiveness in reducing prejudice even in contexts of entrenched conflict where intergroup relations are difficult. Since, however, face-to-face contact is often times unfeasible, not sought, and not pursued, the ability of indirect forms of contact like extended friendships (Wright, Aron, McLaughlin-Volpe, & Ropp, 1997), vicarious contact (Mazziotta, Mummendey, & Wright, 2011), and imagined contact (Crisp & Turner, 2009), to reduce prejudice has been explored. Indirect types of contact were proposed as (at least temporary) alternatives to face-to-face contact and as a means of preparing individuals for actual contact (see e.g., Eller, Abrams, & Gomez, 2012). It was also suggested that indirect contact may even be more beneficial than direct contact because it is less anxiety-provoking, and because group distinctiveness (salience) is sustained during contact (Wright et al., 1997).

There has been very little research comparing direct and indirect types of contact in terms of their prejudice-reducing effects, and this scarce research has focused exclusively on the comparison of naturally occurring contact like direct and extended friendships (see Feddes, Noack, & Rutland, 2009). The existing research suggests that extended friendships are only effective or have more profound effects in reducing prejudice in segregated vs. mixed contexts (Christ, Hewstone, Tausch, Wagner, Voci, Hughes, & Cairns, 2010) and amongst individuals with less prior intergroup contact (Cameron, Rutland, Hossain, & Petley, 2011). There has been no research in the past comparing direct and indirect contact interventions in terms of their effectiveness in bringing about positive intergroup outcomes. The claim that indirect contact is an alternative to direct contact has therefore not, so far, received adequate and comprehensive support. There has been no study in the past comparing indirect forms of

contact, like for example imagined and vicarious contact, in terms of their prejudice-reducing outcomes but also in terms of their ability to prepare individuals for actual face-to-face contact. Lastly, the longitudinal effects of direct and extended friendships were never in the past compared in a low contact context and very little is known about whether these longitudinal effects are mediated by different or similar mechanisms.

This thesis has three main goals: a) to assess and compare the effectiveness of direct and indirect contact interventions (direct, vicarious, and imagined contact) in leading to more favourable outgroup attitudes, less anxiety for future contact, and more willingness for contact in comparison to pretest scores on these variables as well as in comparison to a control (no-contact) condition, b) to more deeply explore the effectiveness of imagined contact in yielding more positive outgroup evaluations and in preparing individuals for future contact, and c) to assess and compare the longitudinal effects of naturally occurring contact (direct and extended friendships) on outgroup attitudes and to examine whether these effects are mediated by the same or different mechanisms. All studies in this thesis were conducted amongst (mainly young) Greek Cypriots in Cyprus, a setting of minimal intergroup contact and long standing segregation.

In Chapter 1, I provide an introduction to Cyprus, and the relations between Greek Cypriots and Turkish Cypriots, the two main communities inhabiting the island. I go back in time to trace the origins of their coexistence when Cyprus was part of the Ottoman Empire, and explain how the growing nationalism first in the Greek Cypriot community and then in the Turkish Cypriot community caused the two communities to pass from a state of relatively peaceful coexistence, to a state of heated conflict that led to the division of the island in 1974. I also explain how the division as well as the complete prohibition of movement through the

Green Line from 1974 to 2003, resulted in the alienation of the two communities, and particularly the youth who were born and raised in a context of outright segregation. I note furthermore that since the opening of the checkpoints in 2003 intergroup contact has been minimal, and that the prevailing idea is that conflict resolution will come as a consequence of the settlement of the political problem. I argue that an unresolved conflict is feeding back to the political problem and end the chapter by proposing that intergroup contact can be a step towards conflict resolution.

Chapter 2 serves as a theoretical introduction to the intergroup contact hypothesis (Allport, 1954) as well as to indirect types of intergroup contact deriving from it. In the first section of Chapter 2, I discuss in detail the origins and some initial support for and criticisms of the contact hypothesis. I also describe how later research on intergroup contact addressed these criticisms and was able to give answers to three main questions pertaining to: a) whether contact works, b) when it works best, and c) how its effects come about. I conclude the first section by outlining issues not (adequately) addressed by existing research. In the second section I present the indirect contact types (extended friendships, vicarious contact, and imagined contact) along with available support for their effectiveness in reducing prejudice. I also highlight pending issues for each of the types of indirect contact. I conclude Chapter 2 by outlining the research questions addressed in this thesis and the studies comprising it.

Chapter 3 consists of three experiments assessing and comparing the effects of different types of contact interventions so as to test whether indirect forms of contact yield prejudice-reducing effects that are comparable to the effects of direct contact in terms of their strength, magnitude, and longevity. The different types of contact are also compared in terms of how they are experienced and perceived by the participants (e.g., how anxiety-provoking

each one is and how salient the group memberships are during the interaction). In Experiment 1 (N = 68), I test the relative effectiveness of direct and vicarious contact in yielding more favourable attitudes at posttest in comparison to pretest. I also test Wright et al.'s (1997) hypothesis that vicarious as opposed to direct contact is less anxiety-provoking and that it leads to higher group salience. I find that both direct and vicarious contact yield more favourable outgroup attitudes at posttest and that these effects are more profound for direct contact. I find no support, however, for Wright et al.'s (1997) postulations.

Experiment 2 (N = 79), comprises a replication as well as an extension of Experiment 1. Direct and vicarious contact are compared against each other and against a control group on the variables of outgroup attitudes and anxiety for future contact. In this experiment, a delayed measurement is also added to check whether contact's effects last a week after the intervention. I find that both direct and vicarious contact lead to more favourable attitudes at posttest in comparison to pretest. These attitudes are significantly more positive than the attitudes reported by the control group at posttest. A week after contact, however, outgroup attitudes regress to pretest levels for both contact conditions. Unlike outgroup attitudes, intergroup anxiety gradually reduces in both direct and vicarious contact to become significantly lower at delayed posttest in comparison to pretest and in comparison to the delayed posttest anxiety reported by the control group. No differences in the strength of these effects were detected between the two contact conditions, and Wright et al.'s postulations are, like in Experiment 1, not given any support.

Experiment 3 (N = 75), aims to replicate Experiment 2 and extends it by including one more type of contact, imagined contact, and additional dependent variables such as, action tendencies and attitudes towards the individual outgroup member. The results show that direct, and to

a weaker extent, vicarious contact lead to more favourable attitudes at posttest which, like in Experiment 2, become less favourable at delayed posttest, and as such, are not different to the participants' attitudes prior to the intervention. Unlike attitudes towards the whole outgroup, attitudes towards the outgroup member remain positive a week after contact across all contact conditions. All types of contact lead to a significant anxiety reduction. At delayed posttest anxiety is lower in comparison to pretest anxiety within each contact condition and between each type of contact and the control group. Finally, action tendencies become significantly more positive at posttest in comparison to pretest only for direct and imagined contact but not for vicarious contact. The effect of direct contact on action tendencies is still traceable a week after contact unlike the effect of imagined contact that fades away with time. The magnitude of the effects of the three types of contact and how the participants in each condition experience the interaction do not significantly differ between types of contact.

Chapter 4 examines further the ability of imagined contact to produce positive intergroup outcomes. The chapter consists of two experiments. In Experiment 4 (N = 60), I explore possible extensions to the 'standard' (positive) imagined contact scenario (see Crisp, Stathi, Turner, & Husnu, 2008) by incorporating into it information about how similar and/or different the two individuals (self and outgroup member) as well as their respective ingroups are. I come up with three imagined contact conditions where the participants are asked to imagine that through the interaction they find out that they are similar ('high similarity' condition), different ('low similarity' condition), or both similar and different ('balanced similarity' condition) to the outgroup member and the outgroup as a whole. Previous research has shown that while interpersonal similarity attracts (see Byrne, 1971), intergroup similarity may have the opposite effect (e.g., Grant, 1993). This is because intergroup similarity can impose a threat to

the distinctiveness of the ingroup. According to Social Identity Theory (Tajfel & Turner, 1986), individuals strive to be members of distinct social groups and they will therefore react to any attempt that puts their group's distinctiveness at risk. On the other hand, a focus on interpersonal/ intergroup differences exclusively is also expected to lead to negative intergroup outcomes (see Jetten & Spears, 2003). My hypothesis is that a condition incorporating both similarities and differences between the two members and their respective ingroups will lead to more favourable outcomes because it buffers against distinctiveness threat but it also satisfies the individuals' need to belong while not losing their uniqueness (see Optimal Distinctiveness Theory, Brewer, 1991). I find, in line with Optimal Distinctiveness Theory, that the 'high' as well as the 'low' similarity conditions produce less favourable outcomes (outgroup attitudes) in comparison to the 'balanced similarity' condition and that they do not significantly differ from each other. In line with Social Identity Theory, the difference in the effects of the 'high' and the 'balanced' similarity conditions is accounted for by differences in distinctiveness threat: individuals in the 'high similarity' condition report feeling more threatened than the participants in the 'balanced similarity' condition.

In Experiment 5 (N = 50), I test the 'balanced similarities' condition against positive imagined contact. Positive imagined contact was thought to encompass those conditions that are both necessary and sufficient to promote positive intergroup outcomes for imagined contact (Crisp et al., 2008). Since imagined contact's main contribution to prejudice reduction has been suggested to be the preparation of individuals for future contact (see Crisp & Turner, 2013), I compare the two imagined contact conditions against each other and against a control condition not only on outgroup attitudes, but also on variables tapping the readiness (intergroup anxiety and contact self-efficacy) of participants to have future contact and their

willingness to pursue it (action tendencies). The results indicate that whereas both positive and ‘balanced similarity’ imagined contact lead to more positive attitudes in comparison to the control condition, only the ‘balanced similarity’ condition leads to higher contact self-efficacy and (marginally) less anxiety for future contact than the control condition. Mediation analyses also show that ‘balanced similarity’ imagined contact (vs. the control condition) has an indirect effect on more positive action tendencies via reduced anxiety for future contact and higher contact self-efficacy (combined). Positive imagined contact on the other hand, is undifferentiated from the control condition on these variables.

Having focused entirely on the examination and the comparison of the effects of contact-based interventions in Chapters 3 and 4, Chapter 5 reports one study (Study 6), which concentrates on the (longitudinal) effects of naturally occurring positive intergroup contact, namely direct and extended friendships, and their mediation. Study 6 ($N = 1000$) employs a three-wave longitudinal design and aims to: assess and compare the temporal effects of direct and extended friendships on outgroup attitudes, check for selection bias (i.e., the possibility that individuals holding more favourable attitudes have more direct and extended friendships), and investigate which of the two mechanisms, intergroup anxiety (a mechanism found to best explain direct contact’s effects on prejudice, see Pettigrew & Tropp, 2008) or ingroup norms (a construct initially proposed as a mechanism accounting for the effects of extended contact on prejudice, see Wright et al., 1997), is better at explaining each friendship type’s effect on attitudes. I find a longitudinal indirect effect of both direct and extended friendships at Time 1 on attitudes at Time 3 via the mediators at Time 2 but no direct or indirect effects extending from Time 1 attitudes to Time 3 direct or extended friendships. The indirect effects of direct friendships on attitudes are mediated by both ingroup norms and intergroup anxiety, whereas

the indirect effects of extended friendships on attitudes are only mediated by intergroup anxiety, and not ingroup norms.

In Chapter 6, I summarize the key findings which I discuss in relation to the theory and the research questions presented in Chapter 2. Overall, the results of the six studies suggest that at least in the Cypriot context, indirect types of contact can lead to short- and longer-term prejudice reducing effects that are also comparable but not, however, superior, to the effects of direct contact. This is the case both for contact interventions as well as naturally occurring contact. There is also support for the claim that indirect types of contact can prepare individuals for future contact. I end Chapter 6 by outlining the limitations of the research presented in this thesis and how future research can overcome these limitations, and by highlighting the challenges of translating research into real-life applications, particularly in fraught intergroup contexts.

TABLE OF CONTENTS

ACKNOWLEDGMENTS.....	2
ABSTRACT	3
EXTENDED ABSTRACT	4
LIST OF TABLES.....	17
LIST OF FIGURES.....	18
CHAPTER 1: Intergroup relations between Greek and Turkish Cypriots in Cyprus.....	20
The chronicles of a conflict.....	21
The Ottoman years (1571 – 1878).....	23
The Christian Orthodox church and the slow rise of Greek Cypriot nationalism in the 19 th century	25
The British rule (1878 – 1960).....	27
The two nationalisms in conflict	30
The anti-colonial struggle of Greek Cypriots and the end of British rule.....	32
A crippled independence	33
The difficult years of 1963 – 1974.....	35
The events of 1974	38
The years of intransigence: 1975 – 2003	39
The unexpected developments of 2003 that rekindled hope for a settlement	41
A missed (?) opportunity in 2004 and the years beyond.....	42
Inter-communal relations from 1974 to date.....	44
The different narratives of Greek and Turkish Cypriots	45
The need for reconciliation	49
CHAPTER 2: A theoretical introduction to intergroup contact theory, and the case of Cyprus	52
The intergroup contact hypothesis.....	54
The origins of the intergroup contact hypothesis	54
Optimal conditions for intergroup contact – when is contact likely to yield positive intergroup outcomes?	56
Allport’s conditions – necessary or facilitating?.....	60
Contact’s “fifth condition” and the rise of cross-group friendships	62

Generalisation issues	63
The Decategorization Model	64
The Common Ingroup Identity Model	66
The Mutual Intergroup Differentiation Model	68
Does contact work? And do its effects generalize?	69
The superiority of cross-group friendships	72
Contact works – but how?	73
Knowledge about the outgroup	76
Empathy	77
Perceived threats to the ingroup	78
Intergroup anxiety	79
Intergroup contact: Pending Issues	81
Selection bias, the casual sequence problem, and other causation-related problems in intergroup contact research	81
Indirect contact: extended friendships	85
When direct contact is unfeasible or unwanted.....	85
The extended contact hypothesis.....	86
Support for and usefulness of extended friendships	90
Extended friendships and their effects on prejudice reduction	91
How do extended friendships work?	92
Extended friendships – pending issues	94
Does the ‘direction of causation problem’ apply to extended friendships?	94
Are extended friendships sufficient for prejudice reduction?	95
Do direct and extended friendships work via the same or different mechanisms.....	97
Beyond extended friendships: vicarious and imagined contact.....	97
Vicarious contact.....	98
Effectiveness of vicarious contact.....	99
Imagined contact	102
Imagined contact’s effectiveness	103
Imagining a positive and intergroup contact: necessary and sufficient conditions for imagined contact	103

Imagined contact as a preparation for actual/direct contact.....	105
Vicarious and imagined contact – pending issues	106
Does vicarious contact prepare individuals for actual contact?	106
Comparison of direct, vicarious, and imagined contact.....	108
Assessing contact interventions	110
Are ‘positive’ and ‘intergroup’ sufficient conditions for imagined contact to produce favourable outcomes?.....	111
Intergroup contact in Cyprus	112
Conclusion and present research.....	115
CHAPTER 3: Comparing the effects of direct and indirect types of contact experimentally	121
Current studies.....	125
Experimental set-up.....	127
Methodological challenges.....	128
Analysing pretest-posttest designs	130
Experiment 1	135
Hypotheses	136
Method	137
Results	144
Discussion	148
Experiment 2.....	150
Hypotheses	152
Method	156
Results	159
Discussion	168
Experiment 3.....	172
Hypotheses	176
Method	180
Results	185
Discussion	199
General discussion	207

Some limitations of the research	213
CHAPTER 4: Elaborating on the effects of imagined contact: incorporating similarities and differences to the imagined contact scenario	219
Experiment 4.....	222
Is similarity a good thing in intergroup encounters?.....	222
Current study	225
Hypotheses	228
Method	228
Results	232
Discussion	236
Experiment 5.....	238
Intergroup anxiety and contact self-efficacy as mediators.....	240
Hypotheses	241
Method	242
Results	245
Discussion	249
General discussion	253
CHAPTER 5: Studying the effects of intergroup friendships on prejudice – a longitudinal study.....	261
Cross-sectional design and the determination of causation	262
The longitudinal design as an antidote to the determination of causation problem.....	265
Cross-sectional studies and tests of mediation.....	266
Testing mediational processes with the longitudinal design.....	268
Studying the longitudinal effects of contact on prejudice.....	270
The longitudinal effects of direct intergroup contact.....	272
Mediation of the effects of cross-group friendships on prejudice in longitudinal studies	276
Longitudinal effects of extended friendships and their mediation.....	279
Study 6: A comparison of the longitudinal effects of extended and direct friendships and their mediation in a low-contact context.....	285
Present study	290
Hypotheses	291

Method	293
Results	296
Bidirectional longitudinal model (Model 6c).....	313
Discussion	320
Are the longitudinal effects of extended friendships comparable to the effects of direct friendships?	322
Are less prejudiced individuals more likely to have direct as well as extended friendships in the long run?.....	325
Are there any differences in the mediation of the longitudinal effects of direct and extended friendship?	326
Study limitations and future research	328
Methodological limitations	330
Conclusion	333
CHAPTER 6: Summary and conclusions	336
Summary of key findings.....	337
The longevity of the effects of intergroup contact	337
The superiority of direct contact for improving intergroup relations.....	339
Can indirect contact prepare individuals for direct contact?.....	341
Mediators of contact: intergroup anxiety, norms, and the promising role of contact self-efficacy	345
Does contact reduce prejudice or does prejudice reduce contact?	347
Limitations of present studies and directions for future studies	348
Operationalisation of vicarious contact and its comparison with imagined contact	349
The benefits and challenges of a delayed posttest.....	350
Individual characteristics as moderators of different contact interventions.....	351
Participants	354
Practical implications.....	356
Concluding remarks	360
REFERENCES	363
APPENDICES.....	407

LIST OF TABLES

<i>Table 3.1:</i> Means and standard deviations of the two experimental conditions on the interaction-related variables.....	147
<i>Table 3.2:</i> Means and standard deviations for all conditions on the variables used to test random allocation	160
<i>Table 3.3:</i> Means and standard deviations of the two experimental conditions on the interaction-related variables.....	162
<i>Table 3.4:</i> Means and standard deviations on all measurements of the dependent variables across three conditions	164
<i>Table 3.5:</i> Means and standard deviations for all conditions on the variables used to test random allocation	186
<i>Table 3.6:</i> Means and standard deviations across the three experimental conditions on the interaction-related variables	188
<i>Table 4.1:</i> Means and standard deviations on all variables for the three conditions.....	233
<i>Table 4.2:</i> Inter-correlations between all variables	233
<i>Table 4.3:</i> Means and standard deviations on all variables for the three conditions.....	246
<i>Table 4.4:</i> Inter-correlations between all variables	246
<i>Table 5.1:</i> Intra-class Correlation Coefficients (ICCs), Standard Error (SEs), and <i>t</i> -values	302
<i>Table 5.2:</i> Scale reliability (Cronbach's α), Mean, and Standard Deviation for each measure at each time point	304
<i>Table 5.3:</i> Model fit and comparisons of autoregressive, unidirectional, and bidirectional longitudinal models	317-8

LIST OF FIGURES

<i>Figure 3.1:</i> Pretest – posttest – delayed scores of each condition on outgroup attitudes	166
<i>Figure 3.2:</i> Pretest – posttest – delayed posttest scores of each condition on intergroup anxiety	167
<i>Figure 3.3:</i> Pretest – posttest – delayed posttest scores of each condition on outgroup attitudes	195
<i>Figure 3.4:</i> Pretest – posttest – delayed posttest scores of each condition on intergroup anxiety	196
<i>Figure 3.5:</i> Pretest – posttest – delayed posttest scores of each condition on action tendencies	198
<i>Figure 4.1:</i> Diagram presenting the paths between condition, proposed mediators (intergroup anxiety and contact self-efficacy), and action tendencies	249
<i>Figure 5.1:</i> A bidirectional model showing the indirect effects of Time 1 direct and extended friendships on Time 3 outgroup attitudes, mediated by ingroup norms (for direct friendships) and intergroup anxiety (for direct and extended friendships) at Time 2 (Model 6c in Table 5.3).....	319

Chapter 1: Intergroup relations between Greek and Turkish Cypriots in Cyprus

Cyprus is the third largest island in the Mediterranean sea after Sicily and Sardinia and is situated in the far south east of Europe, to the west of Asia, and to the north of Africa. It covers an approximate area of 9,251 kilometers. Greek and Turkish Cypriots are the largest two communities living on the island of Cyprus for the last four centuries. Apart from these two main communities which comprise approximately 78% (Greek Cypriots) and 18% (Turkish Cypriots) of the island's population, there are smaller ethnic minorities comprising approximately 4% of the total population which have been represented in Cyprus for generations. The biggest minorities are Maronites and Armenians. Latinos make up approximately 7000 people and there is also a small population of approximately 1000 Roma people (Varnava, Koureas, & Elia, 2009).

The majority of Greek Cypriots are Christian Orthodox and most Turkish Cypriots are Sunni Muslims. Maronites and Latinos are Catholic Christians. Armenians are Christians and belong to the Armenian Apostolic Church. Apart from different religions, these groups speak different languages. Greek Cypriots and Turkish Cypriots speak a strong dialect of Greek (Cypriot Greek) and Turkish (Cypriot Turkish) respectively. As these dialects never acquired the status of a language, the official language of Greek Cypriots is Greek and of Turkish Cypriots, Turkish. The vast majority of Maronites and Latinos now speak Greek, as over the years, they integrated with the Greek Cypriot population. There is, however, a fraction of Maronites today who are bilingual in Greek and Cypriot Maronite Arabic. Almost all Armenians are bilingual in Greek and Armenian.

This chapter has two main aims: the first is to give an account of how the two bigger communities living on the island, Greek and Turkish Cypriots, became the two main actors of a “protracted”, “ethno-national” (Adamides & Constantinou, 2011, pp.242- 243) conflict, more commonly referred to by both insiders and outsiders as “*the Cyprus problem*” (see Papadakis, 1998, p.151), that led to the territorial partition of the island. The second aim is to describe how the relations between Greek and Turkish Cypriots developed particularly after the island’s partition and more specifically how the geographical separation of the two communities has in many ways deepened the divide. The chapter concludes with the proposition that the way forward for the two communities lies in reconciliation, an end that can be reached partly via positive intergroup contact.

The chronicles of a conflict

A long line of rulers succeeded one another in Cyprus: Mycenaeans (20th century-16th century BC), Egyptians and the Hittites (16th century-11th century BC), Phoenicians and Assyrians (11th century-8th century BC), Persians (6th century-4th century BC), Alexander the Great (ca. 330 BC), Ptolemeans (ca. 3rd century-1st century BC), Romans (1st century BC-4th century AD), Byzantines (4th century-12th century AD), the Crusaders (12th-13th century AD), Franks (Lusignans) (till the 15th century AD), Venetians (till the 16th century AD). Christianity made it to Cyprus in about 50 AD, when Saints Paul and Barnabas included Cyprus in one of their missionary journeys. Greek was established as the official language in Cyprus during the Byzantine period.

Greek Cypriots and Turkish Cypriots started to coexist on the island during the Ottoman period (1571–1878 AD) which followed the Venetian rule. This is when the first Muslim population started to inhabit the island. According to most accounts (see Dietzel &

Makrides, 2009) the Christian and the Muslim people (later to be referred to as Greeks and Turks, or Greek Cypriots and Turkish Cypriots) experienced a mostly peaceful coexistence for the greatest part of the Ottoman rule. The first signs of intergroup division were an effect of Greek irredentism¹ that grew larger and affected ethnically Greek people (Hellenes) outside the boundaries of mainland Greece when Greeks of the mainland were about to rise up against the Ottoman rule.

Greek nationalism grew amongst Greek Cypriots when the island was conceded to the British by the Ottomans. Increasingly more divisive policies by the new rulers as well as the emphasis on ethno-national identities highlighted the boundaries between the two ethnic groups. Closer to the end of British rule, Turkish nationalism emerged as a reaction to Greek nationalism and the two communities ended up engaging in hostile rivalries. When the British granted the island its independence Greek and Turkish Cypriots found themselves unable to manage co-ruling a united state. More conflicts sparked, culminating in the military intervention of Turkey who like Greece (and Britain) had been granted intervention rights.

The intervention of Turkey in 1974 led to the territorial division of the island and prohibition of mobility vis-à-vis the demarcation line (border). Since 1974 to the present, multiple rounds of negotiations between the two communities attempting to find a political settlement to *the Cyprus problem* have been fruitless. Instead, the alienation of the two communities due to the long standing segregation has been solidified. A realistic and symbolic fracture to the divide was achieved in 2003, 30 years after the separation, with the partial lifting of the travel restrictions. This allowed the two communities to come into contact with each other once again.

¹ Irredentism. (n.d.). In *Wikipedia*. Retrieved August 23, 2013, from <http://en.wikipedia.org/wiki/Irredentism>: is any position of a state advocating annexation of territories administered by another state on the grounds of common ethnicity or prior historical possession, actual or alleged.

During the same period, the international actors involved in the Cyprus debate tried to take advantage of a rather promising pro-settlement momentum that was developing particularly amongst Turkish Cypriots. The attempt, however, receded. Currently, the two communities continue to live on separate sides of the island but movement between them is allowed. The solution to the political problem is still pending. Next I will provide an overview of each of the aforementioned key periods so as to give a more-detailed background to how conflict resulted between Greek and Turkish Cypriots in Cyprus.

The Ottoman years (1571 – 1878)

Upon becoming part of the Ottoman empire, Cyprus was introduced to a form of governance called the millet system. Via the millet system the people of the island were divided into Muslims and non-Muslims with the Muslim group (millet) being at the top of the hierarchy. Millets were essentially semi-autonomous civil units. Their partial autonomy allowed them to have their own legal, fiscal, and economic functions (Constantinou, 2007). In Cyprus the biggest non-Muslim millet was the one of Orthodox Christians. Orthodox Christians were the largest community on the island, substantially larger than even the Muslim community. Non-Muslims (and to a lesser extent, Muslims) were required to pay heavy taxes to the Sultan (Ottoman leader), but in return received protection of their right to practice their own religion (Jennings, 1993). The (religious) leaders of the millets were given authority and they acted as agents of the Sultan (Constantinou, 2007). Their role essentially was to make sure the taxes were imposed and collected, and they consulted with the Ottoman authorities about preserving the order within their millets (Pollis, 1973).

During the first years of their rule in Cyprus, the Ottomans brought to the island approximately 30,000 Ottoman subjects from other parts of the empire (Anatolia in particular)

as manual labour (Hill, 1952). These people, along with approximately 30,000 Ottoman soldiers who were already based in Cyprus significantly altered the demographics of the island. This inflow of Muslims rendered them the biggest ethno-religious group after the Greek/Orthodox Christians. Muslims ended up comprising approximately 20% of the island's population (Morag, 2004). This meant that in a large number of particularly rural areas (approximately 200 villages) on the island, Orthodox Christians and Muslims lived side by side (Dietzel & Makrides, 2009). Almost all of them were peasants and their occupation was the cultivation of their land.

According to most accounts (see Assmussen, 2001; Kitromilides, 1977; Kyrris, 1976), the cohabitation of the two groups was peaceful as the participation in each other's wedding celebrations and religious festivals attests. Where there were frictions, according to Dietzel and Makrides (2009), these were over resources (e.g., plot boundaries, crops, irrigation) and "not around abstract issues such as ethnicity or national origins [...]" (p. 75). Interestingly, while the Ottomans invested in the peaceful cohabitation of the Muslim and non-Muslim populations on the island, mixed marriages were prohibited to avoid the blend of the communities (Katsiaounis, 1996). In reality though, there was blending between the two communities even in the form of intermarriages (Assmussen, 1996). There were also a significant number of non-Muslims who converted to Islam, and people who observed the rituals of both the Christian as well as the Islamic faith (Morag, 2004). Furthermore, Muslim and Orthodox peasants who suffered greatly under heavy taxation, joined together in numerous occasions to oppose increased taxation and general mistreatment by the religious elites who were in the meantime enjoying considerable wealth (Pollis, 1973; 1996).

The Christian Orthodox church and the slow rise of Greek Cypriot nationalism in the 19th century

As mentioned earlier, the religious leaders of the millets and particularly the Christian Orthodox clergy, were given authorities and power by the Ottomans. The Christian clergy, just like the Muslim elite, was exempted from tax and was allowed to own and rent out land. While on one hand the church was supporting the Ottoman officialdom by making sure the taxes of the Christian orthodox millet were collected and that the order was maintained, the Christian Orthodox subjects of the Ottoman empire saw in the institution of the Church their only protector from unfair treatment by Ottoman officials (Morag, 2004). With this double role, i.e., collaborator of the Ottomans and protector of Christian Orthodox people, the Christian Orthodox church gained enormous power over the years of the Ottoman rule. In fact the Greek Orthodox church became so strong during this period that the archbishop (the elected head of the Orthodox clergy) exercised even more influence on the central Ottoman authorities than did the local Muslim leaders (Hackett, 1901 in Pollis, 1973).

While the Christian Orthodox church was at the peak of its influence in Cyprus at the beginning of the 19th century, a “process of Greek intellectual expansion” (Kitromilides, 1990, p. 3) emerged. The aim was to cultivate the Greek national feeling both within the Hellenic Kingdom as well as in the “eastern periphery of Hellenism” (Kitromilides, 1990, p. 3) which included Cyprus. This movement was becoming stronger as Greece, which was under Ottoman rule since year 1453, was getting prepared to go into a war with the Ottomans for its independence. As Cyprus was considered to be ethnically Greek due to its language, traditions, and Hellenic legacy (Kitromilides, 1990), members of Filiki Eteria (Society of Friends), the most influential secret organization of Greek Orthodox nationalists, approached

the then Archbishop of Cyprus (Kyprianos) to ask for his support to their purpose. Their aim was to oust the Ottoman rule and establish a Greek independent state.

The initial reaction of the Cypriot Christian Orthodox Church to the Greek irredentism was not a favourable one (Morag, 2004; Pollis, 1973). In fact the Christian Orthodox church denounced the nationalist ideals and dismissed them as “Masonic activity” (Katsiaounis, 1996, p. 52). Nationalism, in any case, as a secular ideology which was on the rise in 18th-19th century Europe, imposed a threat to religious elites as it diminished their authority (Pollis, 1993)². Given the abundance of privileges granted to the church by the Ottomans, and how intertwined its religious authority was with its political authority, the negative reaction of the Cypriot Orthodox church to Greek irredentism was not a surprising one.

What came to change the initial position of the church towards Greek nationalism was a shift in the policy of the Ottomans. The Ottoman government abolished all the privileges of the Church after being warned by the local Ottoman Governor that the Orthodox Christians on the island were supportive of the Greek War of Independence which had in the meantime started in 1821, and that they also aspired to taking similar initiatives. In addition to stripping the Church of its privileges, the Ottoman authorities punished the elite of the Christian Orthodox clergy, Archbishop Kyprianos and his bishops, by hanging them (Luke, 1921). The hanging of the religious leaders was only part of the harsh fate the Ottomans had in store for the Christian Orthodox clerics.

This change of line on behalf of the Ottoman rulers resulted in a shift in the stance of the church itself. The Christian Orthodox church assumed a different role albeit still a leading

² It might be worth noting here that even the Orthodox church in Greece was opposed to the Greek War of Independence partly from fear that it would lose the power and the privileges granted to it by the Ottomans (Pollis, 1993). The Patriarch of Constantinople notably vetoed the Greek War of Independence by saying that the Ottoman Empire was ‘God-given’ (Papadopoulos, 1990, cited in Pollis, 1993).

one within the Greek Orthodox community. It was now seen as the guardian of Hellenism on the island. As the Greek War of Independence was successful, Greece was established as an independent state in 1830 and the building of the Greek nation began. The Church started to establish close relationships with the newly formed state and strived to boost the nationalistic sentiments that began to be cultivated on the island. By the end of the Ottoman rule (late 19th century) Greek nationalism was on the rise amongst the Christian orthodox subjects of the Ottoman empire in Cyprus, who started, with the Church at the forefront, to ask for *Enosis* (unification) of the island with Greece. This demand became even louder and even more compelling in the years of the British rule in Cyprus that followed the Ottoman rule.

The British rule (1878 – 1960)

After a three-century long Ottoman rule, the British assumed the administration of the island in 1878 in exchange for military support to the Ottoman authorities during the Russo-Turkish War of that year. When in 1914 the Ottoman Empire decided to break its neutrality towards the warring parties during World War I and to align with the Germans, the British Empire proceeded to unilaterally break the agreement of 1878 with the Ottomans and fully annexed Cyprus to the British Crown. World War I proved to be destructive for the Ottoman Empire. The Treaty of the Sèvres (signed in 1920 between the Allies and the Ottomans) led to the inevitable dissolution of the Ottoman Empire and the birth of the nation-state of Turkey (in 1924) which flourished under the command of Mustafa Kemal Atatürk.

The British rule of the island can be distinguished between the pro-annexation period (1875 – 1914) during which the island was still technically Ottoman and the post-annexation period (1914 – 1960) when the island was officially a British colony. In the first part of their rule the British wished to maintain the millet system but “modernize” it (Constantinou, 2007,

p. 256). The preservation of the millet system meant that the religion-based division of the Cypriot people was maintained. Religious identities, however, were soon replaced by ethnic identities. The two main communities, Orthodox Christians and Muslims, were now referred to by the British as “Greeks” and “Turks” whereas any other community was merely classified as “Others” (Constantinou, 2007, p. 257).

From 1914 the British applied a set of policies aiming ostensibly at achieving equality of all religious/ ethnic groups. Instead, according to Pollis (1973) these practices deepened the division between the communities, particularly the “Greeks” and the “Turks” of Cyprus (p. 579) and they also, “worked against the evolution of a Cypriot nationality as a possible reference group and favored the development of separate relatedness to the Greek and Turkish nationalities” (p. 581-582) . Such a policy, for example, was the introduction of a Legislative Council, a council with an advisory role in which the two communities were represented proportionally. The Greek Cypriot community had nine members and the Turkish Cypriot community had three members (a ratio that roughly reflected the population ratio). There were also six British members who often aligned with the Turkish Cypriot members so as to equalize the votes and leave the decision to the chairman (Beckingham, 1957). Furthermore, the community-members manning the Legislative Council were elected by the two communities in *separate* electoral polls (Constantinou, 2007).

Another change that led to the alienation more than the *ad hoc* division of the two communities was the modernization of the agrarian economy that was established in Cyprus during the Ottoman rule. The modernization of the economy meant urbanisation and for this reason a large number of peasants moved *en bloc* from villages to towns. According to Volkan (Volkan, cited in Morag, 2004) while there were 230 mixed villages in 1911, the number of

mixed villages reduced to 221 ten years later, and to 192 by 1946. Nearer the end of the British rule only 33 mixed villages remained. As Morag (2004) points out, peasants formed the segment of the Greek and Turkish Cypriot population who shared commonalities (notwithstanding their social class). Their move to urban areas was catalytic for intergroup relations on the island. When moving into the urban areas the peasants immediately joined their national groups as towns had always been segregated. Segregation generated suspicion and younger generations, who were in any case severely affected by the nationalistic education they received, ended up having no memories or knowledge of prior peaceful coexistence between the gradually more hostile rival national groups (Morag, 2004).

As education was mentioned above it is worth noting that a goal the British started working towards from the beginning of their rule was to reduce illiteracy amongst Cypriots (Pollis, 1973). By 1918 they had managed to significantly increase the number of elementary and high schools and had to some extent achieved secularization of education which was an entirely religious affair till then. As Rappas (2011) interestingly notes though, colonial institutions (including schools), ended up being used by the two communities as “platforms for the diffusion of nationalism” (p. 59). While the British, according to Pollis (1973), had nothing stopping them from creating common schooling and from encouraging a common curriculum and even a common language³ for the two communities, they, in fact, went the opposite way and established two Boards of Education (one Greek and one Turkish) which controlled the respective school systems. Even more controversially the curricula were made compatible to the curricula of Greece and Turkey, and Greek and Turkish school teachers

³ Pollis (1973) here uses the phrase ‘Cypriot language’ to refer to the Cypriot Greek dialect which was extensively spoken by Turkish Cypriots as well as Greek Cypriots in Cyprus, and which was different from Greek spoken in the mainland in that it had been greatly influenced by other languages, including Turkish.

were imported from Greece and Turkey respectively to teach in the local schools (Pollis, 1996).

Thus, the links of Greek and Turkish Cypriots with their respective national centres (or motherlands, Greece and Turkey) became more direct and consequently, stronger. The stronger the links with the respective motherlands became the more pervasive was the dissemination of nationalistic ideas and the more the divide between the two communities widened. According to Kitromilides (1990), the younger generations of Greek Cypriots “tended gradually to [...] identify politically with Greece – a country they had never seen and of which they had a very vague, if not idealized, conception” (p. 6). This led them to wholeheartedly support and advocate a political union of Cyprus with Greece, an aspiration which was of course disliked and later on counter-attacked by Turkish Cypriots. Schools, in other words, became champions of first Greek and later of Turkish nationalisms, the rise of which had detrimental effects for the relations between the two communities on the island.

The two nationalisms in conflict

When the British assumed control of the island in 1878 the Greek nationalism triggered by the establishment of the Greek state and fuelled by the political aspiration of *Megali Idea* (Great Idea) was rampant (Pollis, 1973). *Megali Idea* was the epitome of Greek irredentism. According to the concept of *Megali Idea*, all areas inhabited by ethnically Greek peoples (including the north of Peloponnesus, Asia Minor, and Cyprus) and still under Ottoman rule were to be re-conquered by Greeks and to be annexed to the newly established Greek state. As mentioned earlier, Greek irredentism found refuge predominantly amongst the Orthodox elite in Cyprus who gradually started to identify themselves as Greeks, and started to demand unification (*Enosis*) of Cyprus with Greece.

In these years, i.e., late 19th century to the beginning of the 20th century, Turkish nationalism was still absent both in the Ottoman empire as well as among Cypriot Muslims. Cypriot Muslims continued to identify themselves as Muslims and not as Turks up until the 1920s, unlike the Christian Orthodox who already started identifying themselves as Greeks at the end of the 19th century. Turkish nationalism only emerged in the early 20th century, with the revolution of the Young Turks against the Ottoman empire in 1908 and the birth of the new Turkish Republic in 1924 (Lewis, 1968). Turkish nationalism has been entirely linked to Mustafa Kemal Atatürk, a Young Turk himself, who became the leader of the Turkish state upon its establishment. Atatürk attempted a set of reforms during the first years of his leadership that aimed at the creation of a modern, Western-oriented, secular Turkish state. For this reason he took measures (better known as reforms) to abolish the presence of Islam in the political, economic, and civic sphere (e.g., by closing down the dervish brotherhoods, turning the Quranic schools into state schools, and by prohibiting the public use of the headscarf and the fez) (Lewis, 1968). His aim was to promote instead what was presumably truly Turkish (Afetinan 1962, cited in Olson, 1985). Turkish Cypriots who in the beginning were indifferent to these reforms were left free to adopt them if they wished (Beckingham, 1957). Gradually, however, a number of Turkish Cypriots were socialized with the Turkish nationalistic ideals and Turkish nationalism finally emerged in Cyprus partly as an imported trend but mostly as a reaction to the Greek nationalism.

The agitation of Greek Cypriots for *Enosis* was in the beginning faced with “caustic indifference” (Rappas, 2011, p. 58) by the British. However, as demands for *Enosis*, started to escalate with Greek Cypriots setting the Governor’s office on fire in 1931, the attitude of the British towards what was seen as a stubborn fixation of Greek Cypriots changed from

“caustic indifference” to “frontal opposition” (Rappas, 2011, p. 58). Given the loyalty of Turkish Cypriots towards the British, the latter tactically aligned with the concerned Turkish Cypriot leaders (Rappas, 2011), who of course appreciated the dangers a potential union with Greece entailed for the minority group of Turkish Cypriots.

Turkish Cypriots, themselves feeling threatened and intimidated by the Greek Cypriot aspirations for a political union with a motherland that was not theirs, opposed *Enosis* with their own quest which they called *Taksim*, i.e., the partition of the island into Greek and Turkish sections so that the Turkish minority were protected. By the 1950s a large fraction of both communities was ideologically aligned with the quest for *Enosis* on the one hand and *Taksim* on the other. As technically, however, the obstacle to the Greek Cypriot quest was the British, the 1950s found Greek Cypriots organising themselves to take militant action against the colonial powers. The church, whose power was somewhat confined by the British for a period, re-emerged as a main actor with Archbishop Makarios III becoming the political leader of Greek Cypriots.

The anti-colonial struggle of Greek Cypriots and the end of British Rule

After many political attempts that aimed at achieving the goal of *Enosis*, Archbishop Makarios III, assisted by the Cyprus-born Greek army General Georgios Grivas, established a guerrilla organisation called EOKA (National Organisation of Cypriot Fighters) which took over the anti-colonial armed struggle with the ultimate goal being *Enosis* with Greece. The armed struggle, which took the form of guerrilla war, took place between 1955 and 1959. EOKA's (violent) action was not restrained to the British forces. EOKA, which under the command of General Grivas became a terrorist organisation, proceeded to kill Turkish Cypriots as well as Greek Cypriot leftists (members of the communist party) (see Papadakis,

1998). Turkish Cypriots, on the one hand, were hired as auxiliaries in the British Security Forces (Bruce, 1985) and automatically formed (lethal) enemies of EOKA. Greek Cypriot leftists, on the other hand, expressed concerns about the timing and the form of the struggle and advocated instead the self-determination of Cyprus. For this reason, they were seen as traitors by EOKA and were either intimidated or killed (Papadakis, 1998). These formed the first inter- (and intra-) ethnic killings.

In response to EOKA, Turkish Cypriots proceeded to establish their own armed organisation in 1957, called TMT (Turkish Resistance Organisation). Like EOKA, TMT was a terrorist organisation that asserted violence as a means towards the attainment of its goal which was to protect Turkish Cypriots from Greek Cypriots, on the one hand, and to partition Cyprus (*Taksim*) on the other. This led to even more interethnic riots and further bloodshed. The anti-colonial struggle, in other words, was transformed into an inter-ethnic struggle. The British, whose role in this conflict was controversial (to say the least), were in need of a solution to this mayhem. The decision to declare Cyprus an independent and sovereign state was taken in 1959 and it was backed by Greece and Turkey.

A crippled independence

In 1959 negotiators from Britain, Greece, and Turkey met first in Zurich and then in London to draft the constitution of the independent Republic of Cyprus. The constitution had a three-fold aim: 1) to guarantee the independence of Cyprus, 2) to protect the rights of the two communities, and 3) to preserve two important British military bases on the south coast of the island (King & Ladbury, 1982). To meet the first goal a Treaty of Guarantee was signed according to which Britain, Greece, and Turkey, would be the guarantor powers of the sovereignty and the territorial integrity of the new state and were responsible for preserving

the state's independence and protecting its constitution (Bruce, 1985). The three guarantor powers were given the right to military intervention when any of the above was at stake and for this reason they were allowed to keep military contingents on the island.

In order to protect the rights of the two communities (second aim), the following tenets were agreed: a) *Enosis* and *Taksim* were prohibited, b) the Republic would have a Greek Cypriot president and a Turkish Cypriot vice president who were both granted the right of veto over decisions of the House of Representatives on issues concerning foreign affairs, defence, and security, c) The Cypriot civil service, security forces, as well as the House of Representatives⁴, would be composed of a 70:30 ratio of Greek Cypriots to Turkish Cypriots, d) the National Guard would be made up of 60 % Greek Cypriots and 40 % Turkish Cypriots, e) two communal chambers would have authority over the imposition of taxes and over other communal matters like religion and education, f) a Supreme Constitutional Court made of one Greek Cypriot, one Turkish Cypriot, and one foreign judge had the power to rule on conflicts in the House of Representatives and the communal chambers, f) the five large towns would be divided into Greek and Turkish municipalities.

As far as British interests were concerned (goal three) Britain secured sovereignty over two military bases covering a territory of 99 square miles. The amount of troops Britain could keep on the island (within the bases) was not subject to any limit.

Once the negotiating parties agreed on the basic principles of the drafted constitution the Greek Cypriot leader, Archbishop Makarios III, the Turkish Cypriot leader, Dr. Fazıl Küçük, the prime minister of Greece, Constantine Caramanlis, the prime minister of Turkey, Adnan Menderes, and Harold Macmillan, Prime Minister of Great Britain signed the Zurich – London agreements in February 1959 which came into effect in August of the same year.

⁴ Each community's representatives were to be elected through separate polls.

This settlement, which was often described as a “realpolitik compromise” or an “unwanted child” (Constantinou, 2010, p. 17), was not meant to last for long. According to Morag (2004), the problem with the new constitution is that it imposed coercive consociationalism. It favoured, in other words, a government structure that “encouraged the maintenance and strengthening of national loyalties at the expense of state patriotism” (p. 617). The first disagreements between the two communities on a number of topics, like the 70:30 ratio⁵ in the civil service, the veto power of the vice-president, and the separate municipalities in the main towns (see Morag, 2004) emerged early on.

The difficult years of 1963 - 1974

In year 1963, the by-then president of the Republic of Cyprus, Archbishop Makarios III, communicated to vice-President Küçük a list of 13 proposed amendments to the constitution including the removal of the constitutional right of veto for the President and Vice President, the modification of the ratio of Greek to Turkish Cypriots in the civil service and security forces in favour of Greek Cypriots, and the establishment of unified municipalities (Morag, 2004). All these points of course favoured the majority (Greek Cypriots) and they naturally caused the outright rejection of the proposal by Turkish Cypriots who felt they were being treated as second-class citizens. What these proposed amendments also caused was the re-emergence of inter-communal violence which brought further bloodshed. The Turkish Cypriot side suffered the greater losses (Papadakis, 1998), as the paramilitary organisations of Greek Cypriots were in full swing and their terrorist action was uncontrollable.

A year later (1964), the Turkish Cypriot members of the parliament resigned, Turkish Cypriots stopped paying taxes, and set up their own administrative cells (enclaves) in parts of

⁵ The 70:30 ratio was deemed unfair by Greek Cypriots as the actual ratio of Greek to Turkish Cypriots in 1960 was 80:20.

the island (King & Ladbury, 1982). Turkish Cypriots started abandoning the very few by then mixed villages to go to the enclaves. All these moves on behalf of the Turkish Cypriots were to a certain extent indoctrinated and by all means supported by Turkey (Morag, 2004). In Nicosia, the capital, the (in)famous Green Line separating the two communities was established in the same year and United Nations forces were called in to monitor inter-communal hostility. Greek Cypriots were left alone to rule the Republic of Cyprus and Turkish Cypriots came under what was called the Provisional Turkish Administration (King & Ladbury, 1982).

The years that followed until 1974 were marked by two main developments. The first development concerned Greek and Turkish Cypriots going into bi-communal negotiations that were nevertheless inconclusive and fruitless. The two sides were simply departing from different understandings or visions of what the form of the state should be. Greek Cypriots (as the proposed amendments of the Greek Cypriot president indicated) desired a unitary state based on the idea of majority rule in which minorities (like Turkish Cypriots) would be respected and granted minority rights (Adams, 1966). Turkish Cypriots, on the other hand, argued that the independent state was built based on a federal idea according to which Greek and Turkish Cypriots formed two separate political entities with equal power and equal rights (Adams, 1966)⁶.

The second development regarded the eruption of intra-communal hostility within the Greek Cypriot community. The division was between fanatical *Enosis* supporters who were displeased with what they perceived as Makarios' shift from the national cause (i.e., *Enosis*) and those for whom *Enosis* was not the primary goal and who supported Makarios. Makarios

⁶ These two different understandings/ visions regarding the form of a united state continue to divide the two communities today.

was indeed by then a bigger proponent of Cyprus' self-determination than *Enosis*. Makarios was particularly supported by the Communist party (AKEL) which at the time regarded *Enosis* as a "desirable" but not a "realistic" goal (Papadakis, 1998, p. 152). The alignment of AKEL and Makarios was not seen with a positive eye particularly by the United States who were deeply concerned about the USSR advancing further into south east Europe⁷. The hostility between the diehard *enotists* and Makarios supporters became even starker with the rise of the U.S.-tolerated Military Junta of the Colonels in Greece in 1967. The supporters of *Enosis* were backed up by the Greek fascist regime and soon a paramilitary organisation called EOKA B, was established under the command of General Grivas who led the first EOKA.

EOKA B's proclaimed goal was to complete the task of (the first) EOKA, namely, the unification of the island with Greece (Papadakis, 1998). EOKA B, supported by Greek army officers, embarked on a series of violent actions involving killing and intimidation of faithful Makarios supporters and the communists (Turkish Cypriot communists included). The peak of EOKA B's destructive course was the organisation and the execution along with the Greek militia of a *coup d'état* with the goal of ousting the president (Makarios). The *coup d'état* took place on the 15th of July 1974 – and it was successful. Makarios was ousted but was lucky to escape assassination. He was replaced by a puppet-leader called Nikos Sampson. It took Turkey five days to react.

⁷ The USSR, just like the Cypriot Communist Party, favoured the self-determination of Cyprus unlike the U.S.A., who preferred to have Cyprus under NATO influence. The U.S.A. favoured a different solution to the inter-ethnic disputes in Cyprus, namely the partition of the island between Greece and Turkey. As Greece and Turkey were both NATO members, the U.S.A. did not want the two countries to go into war with each other over Cyprus and reasoned that partition would be a sensible way forward (Bruce, 1985; Morag, 2004; Pollis, 1996). The U.S.A. furthermore did not like the fact that Makarios was a staunch supporter of Cyprus' non-alliance in the Cold War. They furthermore did not like the fact that he was keeping cordial relations with the Cypriot Communist Party and the USSR. For these reasons, Makarios was often referred to by American diplomats as 'the Red Priest' (Bruce, 1985), 'Castro of the Mediterranean' (Pollis, 1996), or 'Castro in a Cassock' (Dunphy & Bale, 2007).

The events of 1974

Turkey utilized its role as a guarantor power to mount a military intervention in Cyprus as the rights (and the physical existence) of its protégés (Turkish Cypriots) were severely threatened by the nationalist Greek Cypriots who had assumed power via the *coup*. The military operation took place on the 20th of July 1974. The Turkish intervention was completed in two phases. The second phase took place in August 1974. By mid-August the Turkish troops who were resisted very little, if at all, by the disoriented Greek and Greek Cypriot troops, proceeded to occupy approximately 38% of the island before they declared a ceasefire.

The aftermath of the Turkish military operation found Turkish Cypriots and Turks in a victorious position and Greek Cypriots suffering the greatest toll in terms of casualties (dead and missing people) and percentage of refugees. About 160,000 Greek Cypriots were expelled from the north to the south creating a massive refugee problem which was resolved with international financial assistance (Fisher, 2001). Approximately 40,000 Turkish Cypriots fled northwards to the Turkish-ruled section after an agreement on the voluntary regrouping of populations (Fisher, 2001).

The Green Line, initially drawn in Nicosia (in 1964), was now extended throughout the island to partition it into two ethnically homogeneous zones, the Turkish-Cypriot-controlled north and the Greek-Cypriot-controlled south. Peace keeping forces of the United Nations stationed in the buffer zone have patrolled the ceasefire line ever since. The ceasefire line could not be crossed by either Greek or Turkish Cypriots (Demetriou, 2007). This meant that the two communities were now geographically divided and the lack of freedom of movement vis-à-vis the Green Line solidified the physical segregation.

The years of intransigence: 1975 – 2003

In 1975, the north sector was called the Turkish Federated State of Cyprus and was led by Rauf Denktaş, an ultra-national politician with an active role in the Turkish Cypriot paramilitary organisation (TMT), the counterpart of EOKA. In 1977 the two sides managed to reach a compromise at the U.N.-mediated High Level Agreements, on a bi-zonal, bi-communal federation as the basis of the political settlement of the *Cyprus problem* (Adamides & Constantinou, 2011). As the subsequent rounds of negotiations led to a political deadlock, however, the Turkish Cypriot side decided to unilaterally declare the north as a separate entity (state), named the Turkish Republic of Northern Cyprus (TRNC). The UN Security Council Resolution 541 of the same year labelled the TRNC as a “legally invalid” state and called on “states not to recognise any Cypriot state other than the Republic of Cyprus” (p. 15-16). The Republic of Cyprus, however, even though it represents all Cypriots (Greeks and Turks) only has control over the south sector and is staffed only by Greek Cypriots. No state, other than Turkey has recognised TRNC to date. Furthermore, a decision by the European Court of Justice in 1994 (Talmon 2001, in Lacher & Kaymak, 2006), maintained that no produce from the TRNC could be imported to the European Community without certificates from relevant Republic of Cyprus authorities. These decisions of foreign bodies led to the international isolation and the economic sanction of the TRNC whose only ally, Turkey, has been supporting and generously sponsoring it since.

The years between 1974 – 2003 can be described as years of endless political deadlocks and years of stark intransigence. During this period, intransigence was particularly demonstrated by the Turkish Cypriot and the Turkish side who doggedly insisted on accepting

nothing less than a confederation⁸ as a solution to the dispute, despite having agreed on a federal solution some years earlier (Lacher & Kaymak, 2006). During this period, all efforts of Rauf Denktaş, who continued to lead the Turkish Cypriot community, concentrated on achieving the international recognition of the TRNC. Denktaş was particularly fearful that any concessions on behalf of Turkish Cypriots would allow Greek Cypriots to once again rule the fate of Turkish Cypriots (Lacher & Kaymak, 2006).

A major development in the 1990s was the application of Cyprus (internationally represented by the Republic of Cyprus, i.e., the Greek Cypriot side) to join the European Union. By 2002 – 2003, when Cyprus' accession to the European Union was close to completion, the United Nations sought to take advantage of this development to intensify the efforts for a solution to the conflict (Nugent, 2003). Even though the TRNC vehemently opposed the accession negotiations and refused to be part of them (Fisher, 2001), the United Nations and other international actors were perceptive to a growing pro-settlement sentiment amongst Turkish Cypriot people. This pro-solution attitude in the north was triggered by the people's discontent with the problems inflicted by the long-standing political isolation of the north including: unemployment, complete dependence on Turkey, financial mismanagement, and lack of freedom of speech (Demetriou, 2007). In their eyes, a solution to these problems could come through the settlement of the (Cyprus) problem and more specifically through the reunification of the island and the simultaneous accession of both the south and the north in the European Union (Demetriou, 2007).

⁸ In simple terms the difference between federation and confederation is that the former system of federation places emphasis on the power of the central (federal) government over the constituent states (e.g., U.S.A.), whereas confederation refers to an association of sovereign states (e.g., Catalonia - Spain) where power is decentralised.

The growing discontent towards the Denktaş regime, coupled with the now tangible likelihood of the Republic of Cyprus joining the European Union, led Turkish Cypriots to engage in massive demonstrations pressuring the regime for a settlement (Nugent, 2003). At about the same period (late 2002) a new pro-islamist government was elected in Turkey whose foreign policies were marked by more flexibility than the ones of previous governments. The growing momentum of the demonstrations and high-level pressures from Turkey led Denktaş to take a surprising action, i.e., to partially remove the 30-year long travel restrictions thus allowing people from either side to cross the Green Line.

The unexpected developments of 2003 that rekindled hope for a settlement

The border between the north and the south opened on 23rd April 2003. As the decision was abrupt, the first reaction of people, particularly Greek Cypriots, was one of shock, surprise, and confusion especially because no one had any idea of how this decision would be implemented (Demetriou, 2007). Despite the numbness of the first two days, the number of crossings skyrocketed from 5,000 to 45,000 crossings from either side by the third day after the opening (Demetriou, 2007). For people born after 1974 it was the first time to visit the other side of the border and, for the vast majority of them, the first time to merely see an individual from the other side of the line. In the first six months (April to October 2003) there were about 1.5 million visits of Greek Cypriots to the north and about 700,000 visits of Turkish Cypriots to the south (Hadjipavlou, 2007). Crossings became an everyday phenomenon. Visits by individuals to the places they were forced to leave behind in 1974 generated mixed emotions and brought mixed reactions. But under no circumstance was there a violent episode (Hadjipavlou, 2007). This was only good news to pro-solutionists.

Along with the partial lifting of the travel restrictions, one more development ignited the hopes of pro-solution forces. This, was the change of the regime in the North after 20 years of Denktaş' right-wing leadership. Denktaş was succeeded by the candidate of the left-wing party, Mehmet Ali Talat, a pro-solutionist himself whose aspiration was to work towards the re-unification of the island and the accession of Cyprus to the European Union.

Meanwhile, the UN Secretary-General Kofi Annan presented a reunification plan (the Annan Plan) to the two sides which was mutually accepted as the basis of negotiations in November 2002. With Cyprus set to join the European Union on the 1st of May 2004, and given the positive developments in the north, both the United Nations and the European Union were willing to assert as much pressure as they could for a solution to be achieved before Cyprus joined the union. A week-long bi-communal negotiations on the Annan Plan with the participation of the UN Secretary-General himself and the collaboration of Greece and Turkey ended on the 31st March, 2004. The decision was taken for separate and simultaneous referenda to take place in the North and the South on the 24th April 2004, through which Greek and Turkish Cypriots would cast their vote for or against the plan and in this way decide on the future of Cyprus.

A missed (?) opportunity in 2004 and the years beyond

In the referenda of April 2004, 65% of Turkish Cypriots voted in favour of the Annan Plan, but a compelling majority (75%) of Greek Cypriots rejected it. These were results that left the international community (particularly the EU, the UN, and the U.S.A.) dissatisfied and upset with the hitherto less-intransigent Greek Cypriot side (Bryant 2004; Loizos, 2006). According to Bryant (2004) the European observers judged the campaign in the Greek Cypriot

side as anti-democratic. According to some, the campaign on the south was overtaken by the nationalistic discourse that was particularly strong on behalf of the Greek Cypriot president (Bryant, 2004) as well as the Christian Orthodox church who vehemently opposed the plan. Greek Cypriots, on the other hand, have a different version of the story. The majority of them believed that the plan was enforced upon them by the international community and that it was seriously biased in favour of Turkish Cypriots. Multiple analyses attempting to explain these results seem to agree on the most important reason leading the majority of Greek Cypriots to reject the plan, namely security-related concerns. Greek Cypriots were unhappy with the plan's provision for the continuing presence of a great number of Turkish troops on the island (Loizos, 2006). Furthermore, Greek Cypriots seemed to believe that the deal was rushed and that a better (and more just, for them) deal could be struck at a later stage (Loizos, 2006).

The immediate consequences of the vote of the two communities on the referenda depend on the observer's perspective. The Republic of Cyprus, in any case, proceeded to become a full member of the European Union in May 2004. The international community, as a form of reward to Turkish Cypriots, proceeded to ease the embargo against the export of northern Cypriot goods (Bryant, 2004). Given free mobility also, Turkish Cypriots had (as they do today) the opportunity to travel to the south to obtain passports and identity cards of the Republic of Cyprus via which they could freely move within the European Union and enjoy other privileges owned by European citizens. Pro-solutionists and bi-communalists of both sides regarded the rejection of the Annan Plan by the Greek Cypriot side as a missed opportunity (Hadjipavlou, 2007). There was also a certain deal of resentment particularly on behalf of left-wing Turkish Cypriots towards left-wing Greek Cypriots (Hadjipavlou, 2007), as the leadership of the left-wing and traditionally more pro-solution party, AKEL, decided to

reject the plan judging that the timing was bad or that Greek Cypriots were not yet ready for a solution.

The hope for a settlement reignited when, in 2008, the leader of the Greek Cypriot left-wing party, AKEL (Demetris Christofias), who despite his rejectionist stance on the Annan Plan was still regarded as a pro-solutionist, was elected to power. Hopes were even higher due to the fact that Christofias' term in power overlapped with the term of the Turkish Cypriot left-wing and pro-solution leader, Ali Talat. However, despite a promising re-start of negotiations in 2008 the two leaders did not manage to go far. To make the future even gloomier for pro-solutionists, Talat failed to get re-elected in 2009 and was succeeded in power by yet another nationalist leader. Christofias' mandate, on the other hand, judged by many as disappointing at the least, came to an end in 2012. The beginning of 2013 found the Republic of Cyprus severely affected by the global economic crisis. At the moment, worries about the economic recession in the north but even more so in the traditionally more affluent south overshadow other problems including the ever-present *Cyprus problem*.

Inter-communal relations from 1974 to date

The geographical division across ethnic lines which was imposed in 1974 (or even earlier in Nicosia), resulted not only in the physical separation of the two communities but also in their social, emotional, and political alienation (Zembylas, Charalambous, Charalambous, & Kendeou, 2011). As Hadjipavlou (2007) puts it, the Green Line prevented the exchange of goods as well as ideas. It prevented any form of contact between the two communities who became increasingly estranged. This non-communication (see Anastasiou, 2002) provided fertile ground for the cultivation of different social representations within the

two communities particularly with regards to *the Cyprus problem* and the country's history (Makriyianni & Psaltis, 2007; Psaltis, 2011).

The regimes on the two sides of the divide invested in a process of “nation building” (Zembylas et al., 2011, p. 333) that involved the meticulous construction of a collective identity (‘us’) that stressed the “Greekness” (for Greek Cypriots) or the “Turkishness” (for Turkish Cypriots) of the people and the simultaneous fabrication of a despicable “ethnic-other” (‘them’) (Zembylas et al., 2011, p. 333). It also involved promoting an ‘ethnically-correct’ way of perceiving the key events that led to the bi-communal conflict as well as instilling, especially through nationalistic education⁹, a self-laudatory historical narrative.

The different narratives of Greek and Turkish Cypriots

Amongst Greek Cypriots, the prevailing interpretation of *the Cyprus problem* is placing the blame on external factors, namely “international conspiracies driven by Turkish expansionism and American schemes” (Lacher & Kaymak, 2006, p. 151). Less (to no) blame is attributed to Turkish and Greek Cypriots who are perceived as victims of a political game or as “pawns on the eastern Mediterranean/ Middle Eastern chessboard” (Lacher & Kaymak, 2006, p. 151). Indeed, the ethnic-enemy fostered by Greek Cypriots is not Turkish Cypriots but the Turks. According to the narrative which is particularly promoted by school education, Turks are the enemy for they invaded and occupied a section of the island and because they continue to prevent peace from prevailing (Papadakis, 2008; Spyrou, 2002).¹⁰ Turkish

⁹ The diffusion of nationalism in education is particularly prevalent in the content of the educational material used for history education on the two sides (see Papadakis, 2008, for a review of history textbooks in each community).

¹⁰ Spyrou (2002) conducted ethnographic fieldwork in Greek Cypriot schools where he asked primary school children to name a group of people who are very different from “us”. The vast majority of his young respondents mentioned the Turks. When he asked them to write down the opposites of a number of words, including the word “Turks” the most frequent responses were “Cypriots” (equated with “Greek Cypriots”,

Cypriots, on the other hand are perceived by most Greek Cypriots as victims and sufferers (like Greek Cypriots) of Turkey's offensive policies (Spyrou, 2001). Turkish Cypriots are seen as "different kind of people" (Spyrou, 2002, p. 266) in comparison to mainland Turks and they are not thought to be the real problem in Cyprus.¹¹

It should be noted here though that the social representations accompanying the categories 'Turks' and 'Turkish Cypriots' are rather fluid. Spyrou (2002) gives an interesting example when he notes that Greek Cypriot children proceeded to classify the (much hated by Greek Cypriots) Turkish Cypriot leader, Rauf Denktaş, as a Turk (even though he is Turkish Cypriot), precisely because he was seen as "evil" just like the "Turkish occupiers" (Spyrou, 2002, p. 266). The distinction between 'Turks' and 'Turkish Cypriots', however is in fact rarely made (Zembylas & Bekerman, 2008). Often the word *they* is used by Greek Cypriots to describe everyone who lives on the other side of the border (see Zembylas & Bekerman, 2008).¹²

Furthermore, Turkey's military intervention, which Greek Cypriots call "barbaric Turkish invasion" (see Papadakis, 2008, p. 137), had been regarded by Greek Cypriots as an unacceptable, illegal, but above all temporary situation that had to be rectified (Papadakis 1998, 2008). This is why the Greek Cypriot side has since 1974 exerted unrelenting efforts to: a) demonstrate in every way possible the illegality of Turkey's intervention, b) prevent any

"Greeks", "[Christian] Orthodox", and "good" (p. 264). Furthermore, the children described the Turks with very negative adjectives such as "barbarians", "heartless", "dirty", "illiterate", "rapists", and "murderers" (p. 264).

¹¹ Indeed in the study by Spyrou (2002), when Greek Cypriot children were asked whether *all* Turks are bad, many made the qualification between "good" and "bad" Turks. Some, proceeded to distinguish between "Turks" from the mainland and "Turkish Cypriots". Turkish Cypriots were described as "good people who also suffer as a result of the Turkish occupation" (p. 266)

¹² Turkish Cypriots on the other hand use the word *Rum* to refer to Greek Cypriots as opposed to the word *Yunan* which they use to refer to mainland Greeks (Constantinou & Papadakis, 2001). Interestingly, *Rum*, according to Constantinou and Papadakis (2001) was the name used for the Christian Orthodox subjects of the Ottoman Empire and it is currently only employed for Greeks who are currently living in Turkey or for Greek Cypriots.

state from recognising the TRNC, c) raise international awareness about *the Cypriot problem* (which is narrowed down to *the Greek Cypriot problem*), d) stress the temporariness of the *status quo*, and e) assure Greek Cypriots that they will go back to their lost lands (now occupied by Turkey).

Lastly, after 1974 Greek Cypriots adopted an official policy of *rapprochement* (re-approachment) (Constantinou & Papadakis, 2001) coupled with an official narrative of *peaceful coexistence* (see Constantinou & Papadakis, 2001; Papadakis, 1998). The latter refers to the fabricated account that Greek Cypriots and Turkish Cypriots coexisted without any problems for centuries and that this harmonious coexistence was only interrupted by the intervention of the foreign powers and particularly Turkey (Papadakis, 1998). Re-approachment, on the other hand is in theory endorsed by the vast majority of Greek Cypriot political parties. The aim is to show Greek Cypriots' good intentions towards Turkish Cypriots (Constantinou & Papadakis, 2001).

The paradox of the re-approachment policy, however, as was implemented by the Greek Cypriot side, is that while bi-communal initiatives were encouraged, the Greek Cypriot authorities endorsed only “unofficial meetings that do not entail or imply recognition of the Turkish Cypriot polity” (Constantinou & Papadakis, 2001, p. 130).¹³ In other words, while bi-communal initiatives were in theory supported by the Greek Cypriot authorities, they were

¹³ Constantinou and Papadakis (2001) give the example where a group of Greek and Turkish Cypriot academics participating in a common workshop could not meet freely in Cyprus and therefore hosted their workshop at a university in Tel Aviv. Academics had to ensure that they travelled there under their personal capacities and not as professors of the respective Greek Cypriot and Turkish Cypriot universities in order to avoid problems with their universities.

technically made very hard to implement due to the continuous concerns of willingly or unwillingly performing acts that would recognise the state in the north.¹⁴

As for the Turkish Cypriot side, the mainstream historical narrative is one in which Turkish Cypriots have always been victims who managed, however, with the help of motherland Turkey to endure the perpetual siege of Greek Cypriots (Lacher & Kaymak, 2006). There is no fault to be attributed to the Turkish Cypriot nationalism. Any offensive actions Turkish Cypriots (or Turkey for that matter) performed are seen as an act of self-defence whereas Greek Cypriots are presented as the ones who committed the real offences.

The military operation of Turkey in Cyprus is referred to and celebrated as the “Happy Peace Operation” (see Papadakis, 2008, p. 136). The arrival of the Turkish army is portrayed as the salvation of Turkish Cypriots (Papadakis, 2008). For Turkey as well as for Turkish Cypriots the new state of affairs (status quo) as shaped after the intervention of Turkey is not, as Greek Cypriots view it, a temporary situation. It is instead a *de facto* situation that has to be maintained (see King & Ladbury, 1982). In fact Turkey along with the Turkish Cypriot leadership of the time, took immediate steps after the military intervention to maintain this *de facto* situation and convince others of its permanency (King & Ladbury, 1982). For this

¹⁴ This is not to say of course that bi-communal initiatives were entirely hindered by Greek Cypriot authorities. Loizos (2006) outlines prime examples of bi-communal activity on the island that took place even before the opening of the borders in 2003. An example was the implementation of the externally-sponsored ‘Nicosia Master Plan’, an ongoing plan aiming to develop both the north and the south side of the capital with respect to e.g., conservation, water supply, and waste disposal, so that it can function as a united town in the future. The Nicosia Master Plan was implemented with great success when the Greek Cypriot mayor of the south section of Nicosia and the Turkish Cypriot mayor of the north section of Nicosia were both committed bi-communalists. Other examples were the conflict analysis/ problem-solving workshops offered by foreign institutes (e.g., the Canadian Institute for International Peace and Security), and meetings of specialized groups or trade-unionists. The group of bi-communalists on either side has not been large at all, however, and as Loizos (2006) points out quite a few if not all the activities undertaken by bi-communal groups were either highly specialized or quasi-academic in nature. Furthermore, none of these bi-communal initiatives could extend beyond the buffer-zone (which was the only place where bi-communal meetings could take place within the country), internet websites, or locations outside the country due to the mobility restrictions. Even after the partial lifting of these restrictions in 2003, bi-communal activities continue to lack the “organic, pragmatic nature of day-to-day livelihood economics” (Loizos 2006, p. 188) as the two communities still live in segregated settings.

reason, argue King and Ladbury (1982), Turkey paid attention to symbolisms like the renaming of the settlements in the North (including the names of towns, villages, streets, signs), the display of slogans, and the building of monuments.

Re-approachment of course did not form a state policy for the Turkish Cypriot side. In fact, the Turkish Cypriot leader, Rauf Denktaş, during his lengthy stay in power did little else but undermine the significance of bi-communal meetings which he often boycotted (Anastasiou 2002; Constantinou & Papadakis, 2001; Loizos, 2006). According to Loizos (2006), Denktaş was very much wary of the possibility that Turkish Cypriots would through these meetings be influenced away from the communal separatism that he preached. This fear, according to Loizos (2006), stemmed from his (Denktaş') belief that if Turkish Cypriots lived again in mixed communities with Greek Cypriots the latter would once again walk all over them.

The need for reconciliation

The memories of the past are still very vivid amongst Cypriots. This is understandable as the generations who experienced the bi-communal conflict are still alive. Atrocities and violence are hard to witness and even harder to subsequently cope with. The traumatic experience of losing a loved one and the deep grief of letting go of one's home cannot (and in fact should not) be suppressed. There have been no Truth and Reconciliation commissions established in Cyprus (as was the case in conflict-torn countries like South Africa, Rwanda, Cambodia, and Bosnia and Herzegovina) (Hadjipavlou, 2007). There has been no psychological support to people who suffered from witnessing or experiencing war-traumas (Hadjipavlou, 2007) and there have been no apologies given or forgivenesses granted at any level.

It could be argued that separation may have shielded younger generations from experiencing more conflict and that it may have helped the older generations to retreat to their own communities and build their lives afresh, away from conflict and the threatening existence of the *other*. There is by now a sizable segment of the population in both communities who would favour a permanent division (partition) of the island instead of a reunified island (Webster, 2005) so that the two communities can continue unobstructed to lead their lives separately. Whatever the desired solution to *the Cyprus problem* was, is, or will be, however, *settling* a problem is different from *resolving* a problem (see Kelman, 2008), and this can most certainly be applied to political problems too. A conflict is successfully resolved, according to Kelman (2008), when it brings reconciliation. I would argue that the opposite process can also be. In other words, the promotion of a culture of reconciliation, or an ‘ethos of peace’ (Bar Tal, 2000) is vital to successful conflict resolution.

Funnily, the official position by the political elites in Cyprus that is widespread also amongst lay-people is that there can be no reconciliation if the political problem is not first settled (Hadjipavlou, 2007; Zembylas et al., 2011). *The Cyprus problem* is undoubtedly a political problem and few would disagree that formal peace talks are going to be the ones who will have to set the ground for harmonious intergroup relations. However, *the Cyprus problem*, just like the ‘Northern Ireland problem’ or the ‘Israel-Palestine problem’ are problems that are sustained and become intractable at least to some extent because of the people themselves. Part of these problems’ intractability is that the peoples involved in the conflict reach a place where they view each other in negative ways (Hadjipavlou, 2004; 2007). This is largely the case in Cyprus too.

In order to create lasting peace, as Riek, Gaertner, Dovidio, Brewer, Mania, and Lamoreaux (2008) contend, the psychological processes that underlie negative outgroup views, or prejudice in other words, should also be addressed. This is where social psychology's contribution to intergroup conflict lies. Social psychology has sought to, on the one hand, understand the nature of intergroup conflict, and on the other hand, to suggest ways of reducing it (Riek et al., 2008).

Probably the most important single contribution of social psychology to the reduction of intergroup conflict has been Allport's (1954) intergroup contact hypothesis. My thesis work capitalizes on the particular social psychological input to intergroup conflict resolution and constitutes therefore a test of the intergroup contact hypothesis in the Cypriot context. I will now proceed to provide, in the chapter that follows, an introduction to the theory of intergroup contact and the relevant literature, before introducing the reader to the specific objectives of this piece of work.

Chapter 2: A theoretical introduction to intergroup contact theory, and the case of Cyprus

The previous chapter reported that the partition of Cyprus has been regarded by some as a fortunate happening that prevented more violence between the two communities. Such a position reinforces assertions like McGarry and O’Leary’s (1995) according to which “sometimes good fences make good neighbours” (p. 210). It is possibly true that cease-fire lines can serve their purpose which is to stop immediate violence. The Green Line in Cyprus probably enabled the country to shift from a state of “*hot war*” to a state of “*cold peace*” in Miller’s (2001, p. 202-203) terms¹⁵. “*Cold*” as opposed to “*warm*” peace (Miller, 2001, p. 210) though, describes a context where war or other threats of force may not be present but where the conflict is far from being fully resolved.

There does not seem to be much to suggest that permanent separation along ethnic lines in Cyprus has brought *warm peace*. Instead of resolving conflict, segregation has arguably put peace on a prolonged standby mode. Simultaneously, separation, as I explained in the first chapter, merely facilitated the cultivation of opposing narratives in the two sides of the divide from which the perspective of the ‘other’ had been entirely excluded. This has added to the intractability of the inter-communal conflict in Cyprus. Coleman (2003) in his attempt to pinpoint the characteristics of protracted conflicts stated:

“[...] with intractable conflicts, the relations between the parties develop in situations where exclusive social structures limit intergroup contact and isolate the ingroup [...]. This lack of contact facilitates the development of

¹⁵ According to Miller (2001), “*Hot war*” describes a high-conflict context where (as opposed to “*cold war*”) there is actual use of force through offensive or defensive military actions.

abstract, stereotypical images of the other, autistic hostilities and intergroup violence.” (p. 20).

In Cyprus the complete lack of any communication between individuals of the two communities has “kept” as Hadjipavlou (2007) notes, “the citizens of both communities ignorant of each other” (p. 70).

I believe that this is the point where research in conflict resolution meets early research in the social sciences tapping prejudice. That the absence of contact between groups leads to ignorance and the negative evaluation of the outgroup, as Coleman describes above, is not a new idea in the social sciences – particularly social psychology. In his book “The Nature of Prejudice”, Gordon Allport (1954) set out to understand racial prejudice which he defined as “an antipathy based upon a faulty and inflexible generalisation [which can] be felt or expressed [and which] may be directed toward a group as a whole, or toward an individual because he is a member of that group” (p. 9). In this same book, Gordon Allport highlighted the link between segregation (and therefore absence of intergroup contact) and prejudice. He devoted a whole chapter to the review of a number of existent studies investigating the effects of desegregation on intergroup relations that led him to formulate what is known today as the intergroup contact hypothesis.

This chapter consists of two main sections: the first section aims at providing an introduction to Allport’s (1954) intergroup contact hypothesis: how it originated, original support, and some initial criticisms directed against it. I will present how research on intergroup contact addressed these initial criticisms and how as an effect the hypothesis became a full-blown theory. Emphasis will be placed on whether, when, and how intergroup contact (works to) reduce(s) prejudice. I will end the first section by outlining some issues that

remain under-addressed by current research. The second section deals exclusively with indirect types of contact (namely extended friendships, vicarious contact, and imagined contact) which have been seen as the alternative to direct (face-to-face) contact when the latter is not feasible. The effectiveness of each of these types of contact in reducing prejudice will be discussed separately, and pending issues primarily regarding the comparison of these indirect types of contact with direct contact as well as the comparison between different types of indirect contact, will be highlighted. I will bring this chapter to a conclusion by outlining the studies comprising this thesis.

The intergroup contact hypothesis

The origins of the intergroup contact hypothesis

Allport (1954) consulted results from studies investigating desegregation in a number of different settings including educational institutions, neighbourhoods, work settings, and the military. One type of study pertained to what Allport called “knowledge-giving contact” (p. 266). These studies focused on school or university students participating in educational programs which aimed at exposing them to people from different ethnic groups. Van Til and Rath (1944), for example, studied pupils of different ethnic groups who visited another state for a week and while there lived in physical proximity with each other. Their study demonstrated a pre-travel to post-travel decrease in the levels of desired social distance the participants reported towards the rest of the group members (regardless of their ethnic background). Smith (1943) investigated the effects of a series of weekend visits that white students made to the households of individuals of the black community. The results revealed more favourable attitudes towards the outgroup (i.e., black community) both immediately after the intervention as well as after a whole year had elapsed.

Studies on residential contact focused mostly on the evaluation of the effects of mixed versus segregated housing. The study by Kramer (1950), for example, found that white respondents who lived in proximity with black people had more contact with them and were less likely, in comparison to white respondents living further apart from black people, to use negative stereotypes to describe the outgroup. Similarly, white housewives who lived in racially mixed housing projects made more positive evaluations about their black neighbours and showed greater support for mixed housing in comparison to white housewives who lived in segregated housing (Deutsch & Collins, 1951).

Workplace desegregation appeared to have equally positive results as desegregation in the other two settings. Brophy (1946), for example, reported on the desegregation of the Merchant Marine, which in a way forced black and white seamen to work together in ships and to be represented by the same maritime union. The study showed that the more white seamen travelled together with black seamen, the more positive their attitudes towards blacks became. Furthermore, white U.S. soldiers who happened to serve in mixed units with black U.S. soldiers during the Second World War developed more favourable attitudes towards blacks in comparison to white soldiers who served in segregated units (Stouffer, 1949).

Allport was not the first to pay attention to the results of the aforementioned studies. In fact, prior to Allport's 'Nature of Prejudice', the sociologist Robin Williams, was given the mandate to review the existing research on desegregation and evaluate its impact on intergroup relations (see Pettigrew & Tropp, 2006). After reviewing these studies, Williams (1947) came up with a set of propositions (as well as testable hypotheses) regarding ways of improving intergroup relations. In one of his suggestions he proposed that a structured programme of intergroup contact could have beneficial outcomes for intergroup relations. He

in fact went as far as to suggest the nature of intergroup contact he envisaged, which entailed intergroup collaboration towards a common task, which was regarded to be mutually beneficial.

Williams' (1947) work provided a useful frame of reference for Allport, who became the first to formally outline a concrete hypothesis for the prejudice-reducing effects of intergroup contact. Allport (1954) proposed that when members of different social groups come into contact, there is a possibility for prejudice towards one another to be reduced. This straightforward hypothesis, according to Gaertner, Dovidio, and Bachman (1996) developed into "social science's major contribution to reducing intergroup bias and conflict" (p. 272). Not everyone would readily agree with Gaertner et al.'s (1996) comment, however (see Dixon, Tredoux, Durrheim, Finchilescu, & Clark, 2008; Pettigrew, 2008; Pettigrew & Tropp, 2005; for a full account of contact's critics). The original form of the hypothesis as formulated by Allport (1954) seemed to be leaving certain issues unaddressed. A number of critical evaluations of the hypothesis have guided the relevant research which in turn aided the refinement of the hypothesis and its promotion to a full-blown theory (see Brown & Hewstone, 2005; Hewstone, 2003; Swart & Hewstone, 2011). The refined version of the hypothesis appears to be more able to account for the criticisms which were, over the years, made against the contact hypothesis. The major criticisms and advancements will be discussed next.

Optimal conditions for intergroup contact – when is contact likely to yield positive intergroup outcomes?

The formulation of the intergroup contact hypothesis sparked remarkable empirical as well as theoretical interest. As the number of studies of intergroup contact grew, scholars

conducted the first reviews of this body of work (e.g., Amir, 1969, 1976, Cook, 1962, 1964). The first common thread that emerged from these reviews was that contact research tended to be concerned with the question of *whether* intergroup contact reduces prejudice. Cook (1962) remarked that the real question to be asked was “[in] what types of contact situations [...] attitude change of specified types will occur” (p. 76). A few years later, Amir (1969) postulated that contact that takes place under unfavourable, as opposed to favourable conditions, is likely to have negative effects such as increasing intergroup tension and prejudice. He defined unfavourable contact as “unpleasant, involuntary, tension laden contact” (p. 338), and contact where groups have unequal status and are in competition.

Perhaps as a response to the above remarks, research in intergroup contact looked to identify those (favourable) conditions under which intergroup contact should happen in order for its positive effects to be maximized (see Hodson & Hewstone, 2013). An obvious place to start from was the investigation of the usefulness of Allport’s (1954) initial four conditions. When formulating the contact hypothesis, Allport suggested that contact will lead to a reduction of prejudice given that the contact situation is governed by the following conditions: a) groups have equal status, b) there is cooperation between the groups, c) the two groups work towards a common goal, and d) the interaction is supported by authorities, law, and custom.

As expected, those instances of contact which incorporated Allport’s conditions were found to be successful at reducing prejudice. Robin and Preston (1976), for example, interviewed Black and White teachers before and after they had attended a formal training on school desegregation where they had to cooperate with each other and work towards common goals (e.g., jointly teaching children with learning disabilities). This situation incorporated all

of Allport's conditions: all participants were school teachers and therefore had the same status; they cooperated with each other to achieve common goals; and the training itself was supported by the authorities. Pre- and post-intervention tests showed a significant prejudice reduction particularly for the White participants. It was furthermore found that the attitudes of the participants 16 months after the training were more favourable in comparison to the attitudes of teachers who did not take part in the training. Of course, this attitude change could have been a result of the content of the training as opposed to the conditions under which the training was held. Further research, however, helped to consolidate the importance of Allport's conditions.

Support was found for each of Allport's conditions separately. With regards to status equality, Brewer and Kramer (1985) found that when groups entered the contact situation having equal status, this enhanced contact's effects. In an effort to examine contact effects in settings where people naturally have unequal status, Cohen and Lotan (1995) found that contact was facilitated when children of different academic status were encouraged to perceive each other as having equal status. Moody (2001), furthermore, found that interracial mixing in settings which ensured status equality promoted interracial friendships, whereas settings where status equality was not favoured (e.g., in schools that disproportionately assigned minority students to non-academic tracks), were places where fewer cross-race friendships were reported.

The conditions of interdependence and common goals were also given support. A classic example of a well-known and successful intervention built on the basis of these two conditions is the "jigsaw classroom technique" (see Aronson & Patnoe, 1997), where each child in the classroom is meant to become an expert on a single topic that is a vital part of the

larger jigsaw. The aim of the “jigsaw classroom” is to turn a competitive classroom into a cooperative setting (see Aronson, 2000). Walker and Crogan (1998) found that in comparison to mere cooperation the combination of cooperation as well as interdependence, which is endorsed in the Jigsaw classroom environment, was more effecting in enhancing Aboriginal, Asian, and European-Australian children’s (Grade 4-6) self esteem, academic performance, and liking towards school. It also led to increased liking of both ingroup and outgroup peers, and less desired social distance between European-Australian and Asian children.

Even though Walker and Crogan’s (1998) study found the setting encompassing both cooperation and interdependence to yield better results than the setting only encompassing cooperation, other studies have given support to cooperative (as opposed to competitive) settings (see e.g., Brewer & Miller, 1984; Gaertner, Dovidio, Rust, Nier, Banker, & Ward, 1993). Brewer and Miller (1984) more specifically suggested and later found (e.g., Bettencourt, Brewer, Rogers Croack, & Miller, 1992; Miller, Brewer, & Edwards, 1985) that cooperation is particularly successful in improving intergroup relationships when cooperation has an interpersonal orientation (i.e., encouraging ingroup and outgroup individuals in a cooperative setting to explore each other’s unique personal characteristics).

Lastly, intergroup contact that enjoyed the support of authorities was found to have positive intergroup effects. Voci and Hewstone (2003, Study 2), for example, found that contact between Italian workers and non-EU workers at an Italian hospital, the management of which endorsed contact between workers of different countries, led to more positive attitudes towards the outgroup (immigrant workers) and greater support for immigrants’ rights. Support of military (e.g., Landis, Hope, & Day, 1984) and religious (e.g., Parker, 1968) institutions, as

well as authority-figures (e.g., Gomez & Huici, 2008), was found to feature in other successful contact instances too.

Allport's conditions - necessary or facilitating?

While the call was heeded for research on intergroup contact to look into the specifics of the nature of the intergroup interaction which were more likely to yield positive results, research on intergroup contact became rather preoccupied with testing and elaborating Allport's conditions. While support was generated for the optimal conditions set by Allport, researchers started uncovering a number of additional conditions which they deemed important for optimal contact (see Pettigrew, 1998), such as: a) the use of common language by participants in the intergroup contact encounter (Wagner & Machleit, 1986), b) low levels of prior negative attitudes towards the outgroup (Ben-Ari & Amir, 1986), and c) the potential for contact to disconfirm the stereotypical view of the outgroup (Cook, 1978).

Arguably, the overwhelming focus on contact conditions turned out to be unconstructive for the field. Pettigrew (1998), in a thorough and ground breaking review of the intergroup contact hypothesis, made reference to the "independent variable specification problem" (p. 69) through which he touched upon the excessive attention the optimal conditions for intergroup contact received. He noted that the hypothesis was running the danger of becoming an "open-ended laundry list of conditions ever expandable and thus eluding falsification" (p. 69). He remarked that the growing number of conditions was rendering the specification of the independent variable impossible, as the ideal contact situation was becoming much more specific and therefore excluded most contact situations, thereby potentially rendering the intergroup contact hypothesis meaningless (see also, Pettigrew 1986, 1997; Stephan, 1987). He proceeded to make the distinction between

conditions that are “necessary” and conditions that are “facilitating” (p. 70) and noted that most of the aforementioned conditions (including Allport’s) were facilitating but not necessary. He based this assertion on the observation that while contact situations in which Allport’s conditions were violated did not lead to prejudice reduction, not all situations where contact was successful encompassed Allport’s optimal criteria.

Current research is less absorbed with the particular contact conditions and the emphasis has shifted to the dimensions of quality and quantity of contact. In general, contact is regarded to be of good quality when it encompasses some or all of Allport’s conditions and/or when it is not “trivial” or “transient” but “important” and “intimate” instead (Allport, 1954, p. 263 and see also Amir, 1969). Of the two contact dimensions, quality of contact at least intuitively would be expected to lead to better outgroup outcomes as quantity per se does not say much about the nature of the interaction.

Islam and Hewstone (1993) attempted to compare contact quality and quantity to find that each dimension had its own contribution to prejudice, with quality of contact leading to greater effects on outgroup attitudes and quantity of contact having a greater effect on perceived outgroup variability. Other studies have shown that quality more than quantity of contact is more likely to lead to positive outgroup outcomes (e.g., Binder et al., 2009; Eller & Abrams, 2004; Greenland & Brown, 1999). Another approach departing from Allport’s (1954) contention that contact quantity will only matter if contact is of high quality was to investigate the multiplicative effect of contact quantity and quality (e.g., Voci & Hewstone, 2003). There are also studies reporting a very high correlation between contact quality and quantity rendering the two constructs hard to separate (see Al Ramiah, Hewstone, Little, & Lang, 2013). In summary, the preoccupation with contact’s conditions which led the contact

research down a long and ultimately sterile path came to an end, particularly after Pettigrew's (1998) theoretical intervention. Instead, contemporary research has focused on the qualitative and the quantitative dimensions of contact.

Contact's "fifth condition" and the rise of cross-group friendships

Whereas the idea of contact quality has generally not been contested, what comprises good quality contact is not necessarily straightforward. In the late 1990s, Pettigrew (1997, 1998) came up with the "fifth condition of the contact hypothesis" (Pettigrew 1998, p. 79) which he called "opportunity for friendship" (p. 79)¹⁶. This condition basically held that any contact situation which provides the individuals with the opportunity to become friends will yield positive intergroup outcomes. Following the same line of reasoning Pettigrew (1997, 1998) postulated that intergroup contact in the form of cross group friendships comprises the ultimate instance of good quality contact. Cross-group friendships, according to Pettigrew (1997, 1998), encompass most of Allport's conditions over an extended period of time and in a number of settings. In other words, friendships are not a one-off intergroup interaction, and the elements of equal status, cooperation, and common goals that characterize them, potentially do so across a number of different settings and for a long duration.

The first large-scale study of the effects of intergroup friendships on ethnic prejudice was conducted by Pettigrew (1997). Pettigrew utilized seven national probability samples of four countries: France, Great Britain, the Netherlands, and (West)¹⁷ Germany to show that friendships negatively predicted prejudice towards a set of outgroups in each country. Even before Pettigrew's, 1997, study, Herek and Capitanio (1996) demonstrated via a national

¹⁶ It should be noted here that much before Pettigrew, Cook was the first theorist to emphasize the importance of contact providing the participants with "acquaintance potential", i.e., "the opportunity [...] to get to know and understand each other" (Cook, 1962, p. 75).

¹⁷ The surveys were conducted in 1988 when Germany was still split into East and West.

probability sample in the U.S. that heterosexual participants who reported having at least one homosexual friend who had disclosed his/ her sexual orientation to them were found to have significantly more positive attitudes towards gay people in general, in comparison to respondents reporting no friendships with homosexual people.

Cross-group friendships have since been systematically studied and their negative relationship to prejudice extends across a number of outgroups including: lesbian and gay people (e.g., Gelbal & Duyan, 2006; Lemm, 2006; Vonofakou, Hewstone & Voci, 2007), minority and majority ethnic groups (e.g., Binder, Zagefka, Brown, Funke, Kessler, Mummendey, Maquil, et al., 2009; Dhont, VanHiel, De Bolle, & Roets, 2011; Jacobson & Johnson, 2006; Levin, van Laar, & Sidanius, 2003; White, Wootton, Man, Diaz, Rasiah, Swift, & Wilkinson, 2009), stigmatized foreign groups (Van Dick, Wagner, Pettigrew, Christ, et al., 2004; Wagner, van Dick, Pettigrew, & Christ, 2003), religious outgroups with whom direct hostility had been experienced like Catholics and Protestants in Northern Ireland (e.g., Hewstone, Cairns, Voci, Hamberger, & Niens, 2006; Paolini, Hewstone, Cairns, & Voci, 2004; Stringer, Irwing, Giles, McClenahan, Wilson, & Hunter, 2009), White, Black, and Coloured South Africans (e.g., Holtman, Louw, Tredoux, & Carney, 2005; Swart, Hewstone, Christ, & Voci, 2010), and Bosnian Muslims (Bosniaks) and Bosnian Serbs in Bosnia and Herzegovina (Sehajic, Brown, & Castano, 2008).

Generalisation issues

The second common thread identified by initial reviews of contact research was concerned with the generalizability of contact's effects. Cook (1963) noted that contact's prejudice-reduction effects may not generalize from the specific contact situation (e.g., desegregated schools) to other situations (e.g., playground after school hours) (see Minard,

1952; Williams & Ryan, 1954, for specific examples). Amir (1976) noticed, moreover, that contact's positive effects may apply towards the outgroup individual who is involved in the contact situation but not necessarily towards the entire group she or he represents. The generalisation of contact's effects was also underscored in the analysis of Pettigrew (1998) who identified it as an additional problem that remained unaddressed in the initial formulation of the intergroup contact hypothesis. Pettigrew (1998) furthermore pointed to one more type of generalisation, namely the generalisation of contact's effects from the immediate outgroup to other outgroups not directly involved in the interaction.

To the extent that the generalizability question can be seen as a question pertaining to the maximization of the effects of intergroup contact, there are three main models, which were suggested as a response to this quest. These are Brewer and Miller's (1984) decategorization mode, Hewstone and Brown's (1986) Mutual Ingroup differentiation model, and Gaertner, Dovidio, Anastasio, Bachman, and Rust's (1993) Common Ingroup Identity Model. I will proceed to review them in some detail next. Out of these models only the model of Hewstone and Brown (1986; see also Brown & Hewstone, 2005) explicitly dealt with the generalisation of contact's effects (Miller, 2002).

The decategorization model

Brewer and Miller (1984; see also, Brewer & Miller, 1988), contended that for contact to have maximal effects, contact should allow the individuals involved in it to interact in an 'interpersonal'¹⁸ mode where their own unique characteristics stand out instead of their group memberships. In a personalized interaction, according to Brewer and Miller (1984, 1988), the

¹⁸ Note here that the term 'interpersonal' is adopted after a distinction first drawn by Tajfel (1978; see also Brown & Turner, 1981), who highlighted the difference between 'interpersonal' and 'intergroup' interactions. The first type of interactions ('interpersonal') are characterized by an inter-individual orientation whereas in the second type ('intergroup') group memberships prevail at the expense of individuating characteristics.

comparison that takes place is between the self and the other person (i.e., a self-other comparison process) and not a comparison between two members of different groups. Through such a process, note Brewer and Miller, outgroup members are differentiated from each other and they are therefore not seen as one and the same. Outgroup differentiation is a crucial step towards stereotype disconfirmation and elimination. Finally, personalized interactions might repeat themselves in new (intergroup) situations either with the same or even different outgroupers. When the interpersonal approach to an (otherwise) intergroup instance is internalized then the group membership ceases to be the primary basis on which one perceives another person.

Friendships are probably the ultimate form of a personalized (or decategorized) interaction and they provide the first and major line of support for the decategorization model. As presented earlier the effectiveness of friendships in yielding positive outcomes that are also generalizable to the group as a whole as well as to other outgroups (see e.g., Hamberger & Hewstone, 1997; Pettigrew, 1997; and Davies, Tropp, Aron, Pettigrew, & Wright, 2011, for a meta-analysis of friendships' effects on prejudice), has received ample support. Yet, one could claim that friendships' profound prejudice reducing effects are not due to decategorization (i.e., absence of group salience at the time of the interaction as the decategorization model would favour), but because friendships are instances of good quality (intergroup) contact. Binder et al. (2009) who tested the moderating effect of outgroup typicality on the longitudinal effects of friendships on prejudice, found that cross-group friendships had a stronger negative effect on outgroup prejudice under high than under low outgroup typicality. In other words the more typical a member of his/ her social group the outgroup friend was perceived to be, the more profound the effects of the friendship were. Obviously,

decategorization and outgroup typicality cannot go hand in hand as the (out)group has to be salient for one to make a judgment about an individual's 'fit' to that group.

Even though the decategorization model had also gained some support from experimental studies in which a personalized (vs. non personalized) form of contact was introduced among artificially created groups (e.g., Bettencourt et al., 1992), it is hard to see how a personalized form of contact can eliminate group salience in situation in which the groups involved in contact are visibly different (hence salient). In these cases also, a question worth asking is whether group salience *should* be abolished. At times, group memberships are very precious (to the individuals) and their elimination may be unwanted. In these cases a conscious attempt to override group memberships can, as I will discuss in more detail next, bring negative outcomes.

The Common Ingroup Identity Model

The Common Ingroup Identity Model (Gaertner et al., 1993; see also, Gaertner, Mann, Murrell, & Dovidio, 1989) favoured (like Brewer and Miller's model) the dissolution of the initial group categories in order for individuals to recategorize themselves under a new common and overarching category (e.g., Greek Cypriots and Turkish Cypriots seeing themselves as Cypriots). The idea of a common category was not entirely new to the field of contact. Allport had already stressed that while having people working "side by side" could help, the benefit would be even greater if these people saw themselves as members of a greater team (1954, p. 489). According to the Common Ingroup Identity model, when two groups unite under one ingroup, prejudice reduction will occur because of the generation of more positive feelings toward former outgroup members (Gaertner, Rust, Dovidio, Bachman, & Anastasio, 1994, p. 227).

Even though the Common Ingroup Identity Model has accumulated a great deal of support both in laboratory and field studies (e.g., Dovidio, Gaertner, Isen, & Lowrance, 1995; Dovidio, Gaertner, Validzic, Matola, Johnson, & Frazier, 1997, and see Gaertner & Dovidio, 2000, for a review), Dovidio, Gaertner and Validzic (1998) acknowledged that contact situations where a common ingroup identity is endorsed can be perceived as threatening to the distinctiveness of the groups. But as was clarified in the past (see Gaertner, Dovidio, & Bachman, 1996), the recategorization under one inclusive category did not necessarily have to mean that the sub-group categories should be forsaken. In fact, Gaertner et al. (1996) claimed that retaining sub-group identification may be particularly important when it comes to contact between certain groups (e.g., racial groups) whose members' sub-group identities greatly mattered.

Validating the above, Crisp, Stone, and Hall (2006) demonstrated in a series of four studies that an attempt to recategorize the ingroup and the outgroup under one common identity did indeed result in increasing ingroup bias amongst those individuals who identified highly with their ingroup. They also found though, that when distinctiveness threat was alleviated by preserving the salience of the two categories within the superordinate classification (Experiment 4), the negative effects of recategorization on outgroup outcomes were attenuated.

The results of the aforementioned study also provide support for a more integrative model, namely a dual identity model, where both the superordinate category as well as the subgroup categories need to be simultaneously salient for positive outcomes to occur. This latter model, in fact, is a hybrid model incorporating the basic premises of the Common Ingroup Identity Model and the model I am about to describe next, the mutual intergroup

differentiation model which capitalized on the importance of preserving the salience of the categories of the individuals participating in an intergroup interaction.

The mutual intergroup differentiation model

Seen predominantly as an opposing model to the decategorization model of Brewer and Miller (1984), Hewstone and Brown's (1986, 2005) mutual intergroup differentiation model postulated that for contact to lead to positive and generalizable effects, it should be 'intergroup' rather than 'interpersonal'. 'Intergroup' as opposed to 'interpersonal' contact is described by higher group salience which they defined as the: "awareness of group memberships, awareness of group differences, perceived typicality of the out-group member(s) with whom there is contact, perceived homogeneity of the out-group, or different combinations of these" (Brown & Hewstone, 2005, p. 76).

This fairly broad definition of group salience let different studies operationalize group salience in different ways. In survey studies, scales using a number of items tapping different aspects of the construct were mostly utilized. There are survey studies, however, that singled-out one of the facets of salience, like for example outgroup typicality (see earlier example of Binder et al., 2009). In experimental studies too, group salience was manipulated in a number of ways, like for example: making explicit reference to the ethnic background of the outgroup member (see Van Oudenhoven, Groenewoud, & Hewstone, 1996), or stressing the physical differences between the two groups (e.g., Maras & Brown, 2000). Other manipulations of salience included low vs. high outgroup typicality (e.g., Wilder, 1984; Wilder, Simon, & Faith, 1996) or the accentuation (vs. overshadowing) of intergroup differences (e.g., Wolsko, Park, Judd, & Wittenbrink, 2000).

Regardless of the operationalisation of group salience, however, the mutual intergroup differentiation model has received ample support from past studies (see Brown & Hewstone, 2005, for a review). Importantly, support was specifically sought and found for ‘intergroup’ contact’s ability to yield outgroup-member-to-whole-outgroup generalisation (e.g., Brown, Vivian, & Hewstone, 1999; Gonzalez & Brown, 2001; Van Oudenhoven et al, 1996). Moreover, group salience was found to moderate the effects of both quantity and quality of contact (e.g., Brown et al., 1999; Greenland & Brown, 1999; Voci & Hewstone, 2004, and see Brown & Hewstone, 2005, for a review) showing that both contact quality and quantity yielded (greater) prejudice-reduction effects when group salience was higher.

While Hewstone and Brown (1986) initially adopted a hard line about group salience suggesting that contact *has* to preserve group saliency for it to have any effects, they later on (see Brown & Hewstone, 2005) acknowledged the importance of an intimate interaction (which is more likely to be achieved in an interpersonal mode of communication), and moved towards a more integrative model. According to the integrative model, contact will have positive and generalizable effects when both the intergroup (category salience) as well as the interpersonal elements are present during the interaction.

Does contact work? And do its effects generalise?

While it was acknowledged from the very beginning of the formulation of the contact hypothesis that mere contact or contact governed by unfavourable conditions (see Allport, 1954; Amir, 1969) is less likely to yield positive effects and more likely to worsen intergroup relations, none of the narrative analyses of the body of work on contact could provide a straightforward answer to the empirical question of whether contact works and whether its effects generalise. The mammoth task of a statistical meta-analysis on the results of contact

studies was undertaken by Pettigrew and Tropp (2000, 2006). The need for such an analysis became increasingly compelling after multiple critics of the theory cast doubts on the actual effectiveness of intergroup contact to reduce prejudice (e.g., Hopkins, Reicher, & Levine, 1997; McClendon, 1974; Patchen, 1999). One of the strongest criticisms fired against the intergroup contact hypothesis contested the existing support for it by judging it as “premature”, and claimed that the studies investigating intergroup contact’s effects were lacking ecological validity (Ford, 1986, in Pettigrew & Tropp, 2006, p. 752).

Pettigrew and Tropp (2006) in their turn asserted that the previous reviews of contact literature were incomplete as each one of them dealt with a segment of contact studies by narrowing their inquiry down to contact between certain types of groups (e.g., Patchen, 1999) or in specific settings (e.g., Cagle, 1973; Carithers, 1970). They further argued that previous reviews were lax about inclusion rules that resulted in them including into their analyses studies which operationalized contact as intergroup proximity for example. Importantly, claimed Pettigrew and Tropp (2006), previous reviews had not quantitatively assessed contact’s effects but they merely made their judgment for or against the effectiveness of contact based on their own subjective understanding of the relevant literature.

The meta-analysis by Pettigrew and Tropp (2000, 2006) therefore, comprised a quantitative assessment of the overall effect of contact on prejudice based on all the empirical studies published between 1940 to 2000. The studies were chosen based on strict inclusion criteria whereby contact was operationalized as “actual face to face interaction between members of clearly defined groups” (p. 754). These criteria yielded 515 studies, 713 independent samples, and 1383 individual tests. Through this meta-analysis Pettigrew and Tropp (2006) sought to answer key questions such as a) does contact work, b) do its effects

generalise from the contact situation to other situations, from the contact individual to the whole outgroup, and from the immediate outgroup to other outgroups, c) whether Allport's conditions are necessary or facilitating for contact to have positive effects. The main findings of Pettigrew and Tropp's (2006) meta-analysis were as follows:

1) The mean effect size (r) for the relationship between contact and prejudice was $r = -.215$ thus suggesting that intergroup contact generally relates negatively and significantly to prejudice. Of all studies, 94% reported a negative relationship between contact and prejudice¹⁹. It should also be noted here that while in this report of their meta-analytic findings Pettigrew and Tropp (2006) do not make a distinction between majority and minority samples, in their 2005 report, Pettigrew and Tropp, specifically looked into the relative strength of contact's effects for majority vs. minority groups. They found that contact is more effective in reducing prejudice amongst majority group members ($r = -.23$) in comparison to minority group members ($r = -.18$).

2) Results supported a) outgroup-member-to-outgroup generalisation: the average effect of contact on prejudice toward the entire group (mean $r = -.23$) was not significantly different (weaker) than the average effect obtained for the outgroup member with whom there was contact (mean $r = -.21$), b) contact situation-to-other situations generalisation: mean $r = -.24$, and c) outgroup-to-other outgroups generalisation: mean $r = -.19$.

3) For those studies where contact met Allport's optimal conditions, the effect of contact on prejudice was even larger (mean $r = -.29$) than the general mean effect size (i.e., $r = .22$). Contact situations not incorporating Allport's conditions still produced a significant negative relationship with prejudice, even though their effect size (mean $r = -.20$) was smaller than the effect size reported for those studies meeting Allport's criteria.

¹⁹ This was after controlling for selection and publication bias.

These results following rigorous analyses provided robust support for the negative relationship between intergroup contact and prejudice. Furthermore the generalizability of contact's effects was supported. Pettigrew and Tropp (2006) when discussing this result remarked that the operationalisation used for contact (see selection criteria) specifically asked for contact between distinct groups (as opposed to individuals) and noted that Hewstone and Brown's (1985; Brown & Hewstone, 2005) 'intergroup' contact model could well account for why the results of the meta-analysis revealed such strong support for generalisation. Lastly, the meta-analytic results provided support for Pettigrew's (1998) contention that Allport's conditions were facilitating rather than necessary as contact situations not incorporating Allport's conditions yielded a significant negative relationship between contact and prejudice.

The superiority of cross-group friendships

Pettigrew and Tropp's (2006) meta-analysis revealed that the effect of cross-group friendships on prejudice was significant (mean $r = -.25$) and that the effect size yielded by studies incorporating friendships was larger than the effect of other forms of contact (mean $r = -.21$). Davies, Tropp, Aron, Pettigrew, and Wright (2011) performed a meta-analysis specifically on the effects of cross-group friendships on attitudes. They assessed whether the effect size reported by Pettigrew and Tropp (2006) for cross-group friendships could be replicated with a larger sample of friendship studies²⁰ and whether cross-group friendship effects generalised from the outgroup to the outgroup as a whole and to other outgroups.

²⁰ Pettigrew and Tropp's (2006) meta-analysis only included studies published from 1940 to 2000. Davies et al. (2011) meta-analysis included studies published through 2009 thus rendering their sample size over three times larger than that of Pettigrew and Tropp (208 vs. 61 samples).

Their results yielded a mean effect size (mean $r = .24^{21}$) which was comparable to the effect size revealed by Pettigrew and Tropp's (2006) meta-analysis (mean $r = -.25$). The friendships-attitudes relationship furthermore was found to be essentially undifferentiated across different levels of generalisation (outgroup member – outgroup as a whole – outgroups not involved in contact situation) hence supporting the generalisation of friendships' effects. As in Pettigrew and Tropp's (2006) meta-analysis, the negative relationship between friendships and prejudice was found for a number of target outgroups including racial and ethnic outgroups, gays/lesbians, and national and religious outgroups.

Contact works – but how?

When Pettigrew (1997, 1998) expressed the opinion that quite a few of the variables that researchers treated as prerequisite conditions for effective intergroup contact are facilitating and not, in fact, necessary, he also mentioned that some of these variables may be more apt to explain how or why contact works rather than when (see also Stephan & Stephan, 1985). Pettigrew (1998) proceeded to call for a separate focus on the study of the mediation of the relationship between contact and prejudice or, in other words, the delineation of those psychological mechanisms that drive contact's prejudice-reduction effects.

Even though Allport (1954) did not make special reference to the mediators of intergroup contact, the dominant explanation for the effects of intergroup contact on prejudice at that time was that intergroup contact promotes knowledge about the outgroup (see Pettigrew, 1998, 2008). With time, more mechanisms that could potentially explain contact's effects on prejudice were proposed. Stephan (1987), for example, identified stereotype disconfirmation or destruction, the classical conditioning of positive affect, and the extinction

²¹ Note that Davies et al. (2011) examined whether cross-group friendships were associated with more positive attitudes (and not lower prejudice). Therefore the relationship between friendships and attitudes was expected to be positive.

of negative affect as possible mediating processes. Pettigrew (1998) proposed a list of three new mediators in addition to outgroup knowledge. One was behaviour change: Pettigrew suggested, in line with the cognitive dissonance proposition (see Festinger, 1957), that attitudes toward an outgroup change as an effect of having behaved in a positive way towards it (i.e., by partaking in a positive interaction with that outgroup). Intergroup contact therefore forms a change in behaviour which elicits a change in how one feels or thinks about the outgroup. A second proposed mediator, the generation of affective ties, tapped emotional processes and particularly intergroup anxiety (see Stephan & Stephan, 1985) reduction and increase of empathy (see Batson, 1991), as the means through which contact reduces negative outgroup evaluation. The last proposed mediator by Pettigrew was ingroup reappraisal or “deprovincialization” (p. 72, see also Pettigrew, 1997, p. 174) which referred to the process by which one obtains a less provincial perspective on other groups because one’s own group ceases to be the reference point via which one perceives the social world.²²

As the list of proposed mediators got longer, a more recent review by Dovidio et al. (2003) classified these mediators and their own proposed mediators into four categories. The first category of mediators in Dovidio et al.’s (2003) classification was functional mediators. Positive contact, according to Dovidio et al. (2003), can enhance intergroup interdependence which can lead to intergroup attraction, particularly when intergroup cooperation is fruitful and a common goal is achieved.²³ The second category identified was behavioural mediators

²² Deprovincialization has become particularly popular in the last few years as one of the main explanations for intergroup contact’s generalization effects from one outgroup to other outgroups (see Lolliot, Schmid, Hewstone, Al Ramiah, Tausch, & Swart, 2013, for a review). It has received less attention as a mediator of the contact – primary outgroup attitudes relationship.

²³ Whether this factor is a mediator or not is open to discussion. As discussed earlier, interdependence and work towards a common goal were identified as conditions under which contact would be more likely to lead to prejudice reduction (see Allport’s, 1954, optimal conditions). As such, they are moderators and not mediators of contact. Dovidio et al. (2003) seem to claim that given good quality contact, what explains a change in attitudes

and consisted of Pettigrew's (1998) behaviour-change process that I explained earlier. Affective mediators formed the third category of Dovidio et al.'s classification. Under affective mediators, Dovidio et al. (2003) included Pettigrew's (1998) affective ties (i.e., intergroup anxiety and empathy). The fourth category were the cognitive mediators which they divided into two sub-categories: a) learning new information (see Allport, 1954; Pettigrew, 1998), and b) change of social representations.

With regards to the latter (sub) category, social representations, Dovidio et al. (2003) proposed that since social categorization has an active role in creating and sustaining outgroup bias (see the Social Identity Theory of Tajfel and Turner, 1979), then positive contact should have an impact on the relevant processes (of social categorization) and via that an effect on outgroup evaluation. They proposed more specifically that positive intergroup contact can affect social categorization processes, e.g., by encouraging participating individuals to de-emphasize their group identities and proceed to perceive the outgroup member as a (unique) individual (see the decategorization model of Brewer & Miller, 1984), or by encouraging individuals to see themselves and the outgroup member as members of a new overarching category (see the Common Ingroup Identity Theory of Gaertner et al., 1993). A change in the way the outgroup member is perceived, as an effect of positive contact, abolishes, according to Dovidio et al. (2003), the stark ingroup-outgroup distinction which is the cause of outgroup bias. They argue that the attenuation of the ingroup-outgroup mode of categorization, as an effect of intergroup contact, leads to reduced outgroup bias.

During the last decade, attention and therefore support for some of the categories identified by Dovidio et al. (namely, functional mediators, behavioural mediators, and change

is the association of the outgroup with a successful process (interdependence) and outcome (common goal achieved).

in social representations) was meagre. The two prevailing categories of mediators were the affective and cognitive mediators (see Brown & Hewstone, 2005; Pettigrew & Tropp, 2008, and Pettigrew, 2008, for reviews). Of the cognitive mediators, the most popular (and well supported) one has been the attainment of new knowledge about the outgroup (see Allport, 1954; Pettigrew, 1998) whereas of the affective mediators, empathy, intergroup anxiety, and to a lesser extent perceived threat towards the ingroup were more systematically studied.

Pettigrew and Tropp (2008) performed a meta-analysis on the mediators of the effect of intergroup contact on prejudice. For this purpose they tested the significance of three widely studied mediators: knowledge about the outgroup, anxiety reduction, and empathy. The results of the meta-analysis showed that the two affective mediators (intergroup anxiety and empathy) yielded greater size effects than the cognitive mediator (knowledge about the outgroup), but also that the effect size of intergroup anxiety was even greater than that of empathy.²⁴ The aforementioned mediators included in Pettigrew and Tropp's (2008) meta-analysis, along with perceived threats to the ingroup, will be discussed in more detail next.

Knowledge about the outgroup

The idea behind this proposed mediator was very much in line with what Allport (1954) identified as one of the main sources of prejudice, namely ignorance about the outgroup. The reasoning is that if ignorance is the source of prejudice, then contact's positive effect on prejudice reduction is likely to be explained by a change in how much the individual knows about the outgroup. There are some older and some contemporary studies which found support for attainment of (new) knowledge as a mediator of contact's effects on prejudice:

Stephan and Stephan (1984), for example, found that intergroup contact led Anglo students to

²⁴ This is a result that should be interpreted with caution, as the authors point out, as the number of the studies investigating the mediating effects of empathy and new outgroup knowledge were significantly less than the studies researching intergroup anxiety's mediating role.

find out more about a different culture (Chicanos) which, in turn, led them to have more positive attitudes towards the outgroup. More recently, Eller and Abrams (2004) showed in two longitudinal studies that the effects of contact of English students with French students on outgroup prejudice was (partially) mediated by participants' knowledge about the outgroup (Study 1). In their second study too, they found that Mexican nationals working for multinational corporations in Mexico who had more American friends reported less social distance and more positive evaluations of the target outgroup (Americans) as well as a secondary outgroup (Canadians), and that this relationship was (partially) mediated by increased knowledge about the primary outgroup.

Notwithstanding these findings, support for outgroup knowledge as a mediator of contact's effects has not been copious. Harwood (2010) expressed the opinion that knowledge about the outgroup being a cognitive mediator should be more likely to drive contact's effects on cognitive dependent measures such as beliefs and stereotypes as opposed to affective dependent variables such as emotions. Dovidio et al. (2003) too claimed that through contact participating individuals can find out stereotype-disconfirming information which should in turn reduce stereotypes. However, support remains scant for this mediator (something that is also evident from Pettigrew and Tropp's, 2008, meta-analysis of the mediators of intergroup contact) and interest in it, according to Brown and Hewstone (2005), has declined. Instead, additional emphasis has been given to affective mediators of which: a) empathy, b) perceived threats to the ingroup, and c) intergroup anxiety have received considerable empirical support.

Empathy

There are two forms of empathic responding, one is empathic concern which refers to the ability to feel what the other person feels; the other one is perspective taking, which refers

to seeing the world through the other person's eyes (Davis, 1994). Empathic concern is better understood to be the affective dimension of empathy (see Stephan & Finlay, 1999), whereas perspective taking is thought to be the cognitive dimension of empathy (see Batson, Polycarpou, Harmon-Jones, & Inhoff, 1997). These two forms were found, however, by Batson et al. (1997) to be closely associated.

Research to date has given support to the role of perspective taking and, to a lesser extent, of empathic concern in explaining intergroup contact's effects on prejudice. Aberson and Haag (2007), for example, found high quantity and high quality contact of White-American students with African-American students to be associated with increased perspective taking which was, in turn, related to reduced intergroup anxiety which was associated with a reduction in explicit bias. Tam, Hewstone, Harwood, Voci, and Kenworthy (2006, Study 2) on the other hand, found support for the role of both empathic concern as well as perspective taking as mediators of the effect of contact between Catholics and Protestants in Northern Ireland on action tendencies and outgroup attitudes. Also, Swart et al. (2011), provided further support to empathic concern as a mediator. In their longitudinal study, cross-group friendships with white South Africans increased Coloured South Africans' affective empathy which in turn led to more perceived outgroup variability, less negative action tendencies, and more positive attitudes toward white South Africans.

Perceived threats to the ingroup

Threats to the ingroup can be divided into realistic threats (e.g., threats to the group's political and economic power) and symbolic threats (e.g., threats to the group's distinctiveness, value system, or worldview) (see Stephan, Boniecki, Bettencourt, Ervin, & Jackson, 2000; Stephan & Stephan, 2000). Intergroup contact has been found to affect

perceived symbolic as well as realistic threats towards the ingroup. Pettigrew, Christ, Wagner, and Stellmacher (2007) showed that direct friendships with foreigners were negatively associated with symbolic (e.g., threat to freedom and rights) and realistic (e.g., threat to prosperity, economic situation) threat reported by their German participants. They furthermore found that reduced symbolic and realistic threat mediated direct friendships' effects on prejudice. McClaren (2003), using data from a Eurobarometer survey studied the effect of contact between locals and immigrants on anti-immigrant hostility in West European countries (e.g., Belgium, Denmark, France, and Great Britain). She found that contact with the minority immigrant groups reduced threat perception in contexts of high immigration and through that decreased individuals' willingness to espouse anti-immigrant policies.

Al Ramiah, Hewstone, Little, and Lang (2013), in their evaluation of the effects of a nation-building intervention program in Malaysia, found that post-intervention contact among the majority group (Malay) and the minority groups (Indians and Chinese) led the latter two groups to evaluate the majority group more positively, and that this effect was mediated by less perceived symbolic threat. While post-intervention contact led to Malays' ratings of the two minority outgroups being more positive this effect was not mediated by perceived threat.

Intergroup anxiety

Intergroup anxiety (Stephan & Stephan, 1985) refers to the negative emotion accompanying the prospect of having to engage in intergroup contact. Anxiety can stem, according to Stephan and Stephan (1985), from a number of different sources including the concern not to appear prejudiced and that of feeling incompetent, awkward, uncomfortable, and lacking control in an intergroup interaction. Other sources of anxiety pertain to individuals' fear of possibly offending the outgroup member via one's words or deeds or, reversely,

the concern of outgroupers taking advantage of them. Finally intergroup anxiety can also stem from the fear of being judged or rejected by outgroupers as well as ingroupers who do not approve of the interaction.

Anxious people make every effort to avoid contact according to Henderson-King and Nisbett (1996) and in case they find themselves in an intergroup situation, they are more likely than less anxious people to experience heightened psychological arousal. This has the destructive effect of depleting one's cognitive resources, which has repercussions on how one perceives and therefore remembers the outgroup individual and the outgroup by extension. Studies have shown that more anxious people retain more stereotypic information about the outgroup (see Wilder, 1993) and form more affect-congruent (i.e., threatening) impressions of outgroupers (e.g., Curtis & Locke, 2005).

Here therefore lies the importance of good quality intergroup contact (and particularly friendships), where the two participating individuals engage in a positive and reassuring intergroup interaction. A positive intergroup experience can yield intergroup anxiety reduction which in its turn feeds back to how the individual evaluates the outgroup. The role of anxiety (reduction) as a mediator of contact's effects on prejudice has received remarkable attention and abundance of support by relevant research. One of the first and most cited studies investigating the role of intergroup anxiety as a mediator of the contact-prejudice relationship is the study of Islam and Hewstone (1993), who found that contact amongst Hindus and Muslims in Bangladesh was associated with reduced intergroup anxiety which was associated with higher perceived outgroup variability and more positive outgroup attitudes. Similar findings were reported for a number of ingroup-outgroup and contact settings. Intergroup anxiety was found to mediate contact's effects, e.g., for Catholics and Protestants in Northern

Ireland (Paolini et al., 2004), for Colored South Africans and their attitudes, perceived variability, and negative action tendencies towards White South Africans (Swart et al., 2011), for heterosexuals and their attitudes, attitude accessibility, and meta-attitudinal strength towards gay men (Vonofakou et al., 2007), for British adolescents and their attitudes towards South East Asians (Turner, Hewstone, & Voci, 2007a), and for English, German, and Belgian school students and their prejudice towards ethnic minorities living in their respective countries (Binder et al., 2009).

Intergroup contact: pending issues

Selection bias, the causal sequence problem, and other causation-related problems in intergroup contact research

While the negative association between contact and prejudice received ample support which can hardly be contested, particularly after the findings of the very thorough meta-analyses of Pettigrew and Tropp (2006) and Davies et al. (2011), a pending and relatively under-addressed problem has been selection bias and by extension, “the causal sequence problem” (Pettigrew, 1998, p. 69). Selection bias refers to the possibility that the people who engage in intergroup contact are those people who are initially less prejudiced. This possibility was, in fact, acknowledged early on, for example by Amir (1969) who posed the following question: “in a number of studies a relationship was found between the contacts with an ethnic group and a positive attitude change toward this group. But which came first, the contact or the favourable attitude?” (p. 338).

Pettigrew, in his 1998 review of the intergroup contact hypothesis, re-addressed the selection bias problem. He noted that the likelihood of selection bias rendered findings

coming from cross-sectional studies in particular, hard to interpret. Cross-sectional studies are able to inform one whether contact is associated with prejudice but can say nothing about the direction of the causation. Given the selection bias possibility therefore, the likelihood exists that the relationship between contact and prejudice can be better explained by the causal path from prejudice to contact (i.e., more prejudice leads to less contact, or less prejudice to more contact) than by the path from contact to prejudice (i.e., contact reducing prejudice) as the intergroup contact hypothesis maintains.

A simple and straightforward way of addressing the question of causation as well as the selection bias problem is through experiments. Experiments form the most apt method to establish relations of cause and effect and they furthermore eliminate choice via the randomization of the participants to condition. Experiments have been employed by researchers in the field of intergroup contact a number of times in the past (e.g., Cook, 1969, 1978, 1984; Van Oudenhoven et al., 1996) and support is generally found via them for the ability of contact to reduce prejudice (see Hewstone, 2003; Pettigrew, 1998; Pettigrew & Tropp, 2006). Despite their usefulness towards this end, experiments have been the least employed method in the field²⁵. The most commonly used method by contrast, is survey studies which come with the benefits of: a) the utilization of larger (and potentially representative) samples and hence the ability to generalize findings to a larger segment of the population under study, b) more ecological validity, and c) being relatively buffered against task demand characteristics.

Pettigrew (1998) suggested three ways of addressing the question of causality and its direction when survey studies are used. The first one was to study the effects of intergroup

²⁵ Of the 515 studies used in the meta-analysis of Pettigrew and tropp (2006) only 5% were true (as oppose to quasi-) experiments.

contact in contexts where there is no choice given to participants on whether to participate in the intergroup interaction or not. Along these lines Pettigrew and Tropp (2006), in their meta-analysis, assessed whether the effect size of the association between contact and prejudice differed depending on whether the study allowed the participants to freely choose whether to have contact or not. Their findings showed that for studies in which participants were not given a choice as to whether to have contact or not, the effect size of the negative relationship between contact and prejudice was even larger (mean $r = -.28$) than the effect sizes of the pool of studies in which participants had some (mean $r = -.19$) or full (mean $r = -.22$) choice on whether to participate in the contact situation. These findings provided support for the contact-reducing-prejudice causal sequence since had selection bias been influencing the results, then the studies in which participants were freely choosing whether to engage in contact would have yielded a larger association between contact and prejudice.

The second suggestion of Pettigrew (1998) was to apply specific statistical methods (e.g., nonrecursive structural equation modeling) to cross-sectional data which will allow for both paths (the one from contact to prejudice and the one from prejudice to contact) to be simultaneously tested (and compared). Pettigrew had already used these statistical methods in his 1997 paper (see above) where he utilized cross-sectional data from different countries to assess the effect of friendships on prejudice. For the purposes of this paper, Pettigrew (1997) tested and compared the two alternative paths (i.e., from friendships to prejudice and from prejudice to friendship) using nonrecursive structural equation modeling. He found evidence for the existence of both paths but the comparison of these paths showed that the path from friendships to prejudice was stronger than the path from prejudice to contact. This method was

subsequently adopted by studies utilizing cross-sectional data, and I will critically evaluate it in one of the forthcoming empirical chapters (Chapter 5).

The third method for overcoming the causal sequence problem, according to Pettigrew (1998), is longitudinal research. In a longitudinal design, time elapses between the first and the second measurement taken by the participants. In this way, the measurement of variable X precedes the measurement of variable Y which allows for temporal precedence to be properly tested and therefore established. Pettigrew (1998) acknowledged that the longitudinal design solution formed the best method for addressing the causal sequence problem but noted that there were few longitudinal studies in the contact literature.

A series of longitudinal studies has since emerged. In fact, Davies et al. (2011) who performed a meta-analysis of studies entailing friendships, had at their disposal enough longitudinal studies ($N = 25$) which they singled out and analyzed separately. They found that the effect size of the relationship between friendships and attitudes for the group of longitudinal studies (mean $r = .23$) was as large as the effect size found for cross-sectional studies (mean $r = .24$). Even though it is not good practice to compare the results of longitudinal with the results of cross-sectional studies, this lack of difference in the effect sizes seems to at least suggest that a path runs from friendship to prejudice. Had the case been different, i.e., had the association between friendships and prejudice been caused by the reverse path (leading from prejudice to friendships), the effect size for the longitudinal studies is likely to have been significantly smaller.

The causal sequence problem goes beyond the contact-prejudice relationship and extends to the test of mediators between contact and prejudice. Mediation essentially results from a product of two causations; the causal effect of independent variable X on mediator M,

and the causal effect of mediator M on the dependent variable Y. Despite growing research on the mediation of contact's effects (see Tausch & Hewstone, 2010), mediation has predominantly been tested in cross-sectional studies. It is risky to make directional claims on the basis of cross-sectional data, for example, that contact reduces intergroup anxiety and that reduced intergroup anxiety reduces prejudice, since the reverse paths may be true: i.e., less prejudiced people have less anxiety and less anxiety leads to more contact. As most of the existing longitudinal literature has focused exclusively on the relationship between contact and prejudice (see Swart et al., 2011), more longitudinal studies are needed in order to properly explore the psychological processes via which contact works.

Indirect contact: Extended friendships

When direct contact is unfeasible or unwanted

Opportunity for direct contact, according to Wagner et al. (2003), comprises a distal indicator of prejudice. By this they mean that opportunity for contact is likely to yield lower prejudice via increasing the likelihood for one to have more contact as well as more intensive intergroup experiences (like cross-group friendships). In this pathway, contact per se, and particularly cross-group friendships, comprise the proximal indicator of prejudice (see also Van Dick et al., 2002; Wagner, Hewstone, & Machleit, 1989). Wagner et al. (2003) found that differences in prejudice towards ethnic minority members between East and West Germans were explained by a difference in available contact opportunities between the two settings (West and East Germany). More opportunities for contact in West as opposed to East Germany led to more frequent as well as more intense intergroup encounters with outgroupers

for West Germans, who also rated intergroup contact as more important, a stance that negatively predicted outgroup prejudice.

Thus opportunity for contact is vital for direct contact to occur. Opportunity for contact varies significantly between contexts: e.g., segregated vs. non-segregated neighbourhoods, integrated vs. non-integrated educational institutions, work-spaces, and housing, urban vs. rural areas, existence of physical borders vs. no borders between groups experiencing or having experienced ethno-political tension. In settings where opportunity for contact does not exist or is limited then direct contact is unfeasible or unlikely.

Furthermore, even when opportunities for contact emerge, prior conflict between groups may render direct contact psychologically difficult for individuals. As Stephan and Stephan (1985) noted, negative affective states like intergroup anxiety may prevent people from engaging in what they tend to perceive as an uncomfortable, awkward, and even threatening situation where they have to face an outgroup member. Also, alienation from the outgroup may lead individuals who grew up in a context of segregation to be disinterested and not particularly eager to meet the outgroup.

To put it simply, practical obstacles (e.g., segregation), psychological obstacles (e.g., anxiety), or mere disinterest which often accompany intergroup conflicts (particularly the protracted ones) may, and often-times do, render intergroup contact unfeasible, unlikely, or unwanted. If, however, intergroup contact is as central to prejudice reduction as findings of intergroup contact research suggest, then what happens in these situations in which direct intergroup contact is for one reason or the other absent? More recent developments in the field of intergroup contact provide an answer to this question via the exploration of the idea that merely knowing/ learning about or observing a positive interaction between an ingroup member and

an outgroup member will also reduce prejudice (see Christ, Hewstone, Tausch, Wagner, Voci, Hughes, & Cairns, 2010; Dovidio et al., 2011; Hewstone & Swart, 2011).

The extended contact hypothesis

Wright et al. (1997) pointed out that while cross-group friendships are a very promising form of good quality direct contact, they do not meet the requirements set by the mutual ingroup differentiation model of Hewstone and Brown (1986) with regards to group salience. Hewstone and Brown (1986), as I explained earlier, highlighted the importance of maintaining group salience for the effects of intergroup contact to generalise to the group as a whole. According to Wright et al. (1997) cross-group friendships form an instance of interpersonal contact where group salience is low and where therefore the effectiveness of friendships in reducing prejudice towards the outgroup as a whole is at risk. The problem with group salience on the other hand, noted Wright et al. (1997), is that there are occasions when the salience of group memberships can be anxiety-provoking and thus debilitating (see Stephan & Stephan, 1985). Wright et al. (1997) proposed extended contact as a solution to this “conundrum” (p. 74).

Wright et al.’s (1997) extended contact hypothesis postulated that: “knowledge that an ingroup member has a close relationship with an outgroup member can lead to more positive intergroup attitudes” (p. 74). According to Wright et al. (1997), there can be benefits in having a friend who has an outgroup friend in comparison to having an outgroup friend oneself. These benefits are the following: a) the positive effects of intergroup friendships can be retained while group salience is also preserved: this is because the observer of the positive intergroup experience is more likely to be aware of the group memberships of the individuals engaged in the friendship instance because he or she is distant from the situation; b) since

salience is preserved, the positive outcomes of intergroup friendship will be more likely to generalise to the whole outgroup in comparison to the case of direct friendships where the salience is low; c) the effects of intergroup friendships are retained without having to expose oneself to a potentially uncomfortable high-salience situation (as can be the case with direct contact). This means that observing or learning about an intergroup interaction is done in an anxiety-free environment where salience of the two groups is nevertheless high; and d) in the case that extended contact is indeed effective in reducing prejudice then its benefit over direct contact is that it can have a much wider (far-reaching) impact as not every person has to directly know or be friends with an outgroup member in order to reap the prejudice-reducing effects of contact. Instead one person who has outgroup friends can 'spread the word' to many others.

Wright et al. (1997) were the first to test their own hypothesis in a series of four studies, of which two were cross-sectional, and two were experimental. In their first two studies they measured participants' knowledge of cross-group friendships and their outgroup attitudes. They included also measures of direct friendships and of attitudes towards a number of unrelated outgroups so that they could control for dispositional attitudes, dispositional friendliness, and their interaction. In the first study, the ingroup were white Americans and the target-groups were Asian Americans, African Americans, and Latinos. The main findings of Study 1 were: 1) knowing at least one ingroup member who had an outgroup friend was negatively associated with subtle prejudice and positively associated with favourable attitudes towards the particular target outgroup, 2) the more extended friendships the participants reported the lower their subtle prejudice and the more positive their outgroup attitudes were,

3) these associations were retained even after controlling for the respondent's own outgroup friendships and attitudes towards other groups.

Study 2 cross-validated Study 1 results with another sample of white American respondents and a sample of Asian Americans, African Americans, and Latinos. Study 2, like Study 1, yielded a positive association between extended friendships and favourable outgroup attitudes and a negative association between extended friendships and subtle prejudice. This was the case for both the majority group (white American) participants as well as for the minority groups (Asian Americans, African Americans, and Latinos).

Wright et al. (1997) proceeded to conduct two experimental studies which would help them establish the causal effect between extended contact and prejudice. In Study 3, they constructed an intergroup conflict procedure where they first induced conflict between two groups and then introduced an intervention entailing the creation of cross-group friendships for only a sub-set of the two groups' members. They then had these members of the groups who had formed cross-group friendships rejoin their ingroups and describe their (positive) experiences with the outgroup members to their ingroup members (who did not experience the cross-group friendship manipulation). The measures of the members to whom cross-group friendships were communicated (i.e., the extended contact participants) were taken before and after the extended contact intervention. The results of the study showed that there were pre-to-post intervention differences in the (extended contact) participants' ingroup favouritism, outgroup attitudes, and assessment of intergroup relations.

In Study 4, Wright et al. (1997) categorised individuals into groups ostensibly based on their performance in an estimation task.²⁶ They then arranged that the participants observed

²⁶ The process was based on the minimal group paradigm (Tajfel, Billig, Bundy, & Flament, 1971) where artificial categorisation of individuals into groups based on an arbitrary criterion resulted in ingroup favouritism.

an ingrouper and an outgroup, who were in fact confederates, interacting in order to solve a puzzle task. The two individuals (confederates) were instructed to engage in an interaction that was designed to be perceived by the observers as an interaction between either: a) close friends (positive), b) strangers (neutral), or c) disliked acquaintances (negative). Participants, who were randomly assigned to one of the three conditions, were then asked to complete measures tapping their evaluation of the outgroup. The findings revealed that participants in the 'close friends' condition evaluated the outgroup more positively in comparison to participants in the other two conditions.

Support for and usefulness of extended friendships

The studies following the postulation of the extended contact hypothesis have operationalized extended contact as extended friendships, asking participants to report how many ingroup members (normally friends, relatives, and colleagues) they know who have outgroup friends. Even though Wright et al (1997) did not make the distinction then, recent narrative reviews of extended contact (see Dovidio et al, 2011; Harwood, 2010), draw the line between *knowing* an ingroup member who has an outgroup friend (e.g., Studies 1-3 of Wright et al., 1997) and *observing* an ingrouper having a friendly interaction with an outgroup (e.g., Study 4 of Wright et al., 1997). They labeled the former as instances of extended contact, specifically extended friendships, and the latter, as vicarious contact. In this section I will focus on extended friendships and in a subsequent section I will present and discuss vicarious contact.

Extended friendships and their effects on prejudice reduction

Before moving on to present recent research on extended friendships, I should clarify that in studies investigating extended friendships, researchers typically measure direct

friendships and/ or direct contact (quality and quantity) alongside extended friendships. This allows them to control for direct friendships (and/ or contact) and therefore establish that the effectiveness of extended friendships in reducing prejudice goes over-and-above the effect of direct friendships/contact.

Evidence from research investigating the effects of extended friendships has been compelling. Extended friendships were found to be associated with lower outgroup prejudice for: Whites and their prejudice towards South Asians in England (Turner et al., 2007a, Studies 2 and 3), and towards other ethnic minorities including Blacks and Latinos in the U.S. (Schofield, Hausmann, & Woods, 2010), for German adults (Pettigrew et al., 2007), Belgian adults (Dhont, Roets, & van Hiel, 2011), Spanish high school students (Gomez, Tropp, & Fernandez, 2011), Norwegian high school students (De Tezanos-Pinto, Bratt, & Brown, 2010), and Dutch students (Munniksma, Stark, Verkuyten, Flache, & Veenstra, 2013) and their prejudice towards stigmatized foreign groups in their respective countries.

Importantly, extended friendships' effectiveness in reducing prejudice was demonstrated in contexts of entrenched hostility and conflict. Extended friendships between Catholics and Protestants in Northern Ireland were negatively associated with outgroup prejudice (e.g., Paolini et al., 2004; Tam, Hewstone, Kenworthy, & Cairns, 2009; Tausch, Hewstone, Schmid, Hughes, & Cairns, 2011). The same was found for Kosovar Albanians whose extended friendships with Serbians in Kosovo were related to lower prejudice towards the outgroup (Andrighetto, Mari, Volpato, & Behluli, 2012).

In fact, recent studies that investigated the moderation of the effects of extended friendships have revealed that extended friendships are even more effective in reducing prejudice in segregated (as opposed to mixed) settings (see Christ et al., 2010), where direct

contact is more limited and where therefore extended friendships provide the only means of establishing a positive connection to the outgroup. Complementing this finding, research has shown that extended friendships have an even greater positive impact for people with less good quality direct contact (see Cameron, Rutland, Hossain, & Petley, 2011)²⁷ or for people with less or no direct friendships (Dhont & van Hiel, 2011). Very recently, the study of Munniksma et al. (2013) demonstrated a more profound effect of extended friendships for people with more unfavourable initial outgroup attitudes.

How do extended friendships work?

When Wright et al. (1997) presented their hypothesis of extended contact they also outlined four main mechanisms via which extended contact reduces prejudice.²⁸ These were:

- a) Intergroup anxiety reduction: knowing of or observing a positive (and comfortable) intergroup interaction between an ingrouper and an outgroup member will, according to Wright et al. (1997), ease the negative affect (fear and anxiety) related to the idea of an intergroup encounter for the observer of this interaction.
- b) The generation of more positive ingroup and outgroup norms: by Wright et al.'s (1997) reasoning, having vicarious knowledge of an intergroup interaction makes one more aware of the intergroup nature of the interaction in comparison to the instances in which the individual takes part in intergroup interactions. This results in the ingroup and the outgroup becoming, for the person who is vicariously experiencing an intergroup interaction, sources of information with regards to how the two groups view

²⁷ The quality difference refers to individuals who report having outgroup acquaintances as opposed to having outgroup friends

²⁸ Wright et al (1997) also made reference to another possible mechanism, well known from the direct contact literature, namely ignorance reduction (or in other words, learning new information about the outgroup). However, this mechanism was paid little attention by Wright et al. (1997) and by subsequent researchers investigating the mediators of extended friendship (see Eller et al., 2011; 2012, for exceptions).

intergroup communication. Seeing an ingroup member being on good terms with the outgroup is likely to lead one to believe that the ingroup is either in support of interactions with the outgroup or that at least it does not oppose them. The same inferences can be made about the outgroup.

- c) Inclusion of other in self (see Aron, Aron, & Smollan, 1992): Wright et al. (1997) argued that observing an ingrouper interacting with and getting closer to an outgroup member will also bring the outgroup member (and by extension the outgroup), closer to the observer.

These four mechanisms, intergroup anxiety, ingroup and outgroup norms, and inclusion of other in self, were tested by Turner, Hewstone, Voci, and Vonofakou (2008) in two independent studies with White British university (Study 1) and high school (Study 2) students. Their results revealed that controlling for direct friendships, extended friendships with South Asians were associated with more positive outgroup attitudes and that these effects were independently mediated in both studies by all four proposed mediators. Gomez et al. (2011), using two samples, one of a majority (Spaniards) and one of a minority (immigrant group), found, like Turner et al. (2008), that extended friendships were associated with more positive attitudes towards the outgroup (immigrants and Spaniards respectively) as well as more positive expectancies for future direct interactions with the outgroup, while controlling for direct friendships as well as direct contact quantity and quality. The effects of extended friendships on both attitudes and intergroup expectancies were mediated by all four proposed mechanisms.

Other studies obtained further support for the mediational roles of intergroup anxiety reduction (e.g., De Tezanos-Pinto et al., 2010, Paolini et al., 2004) and change in ingroup (e.g., De Tezanos-Pinto et al., 2010) and outgroup (e.g., Eller et al., 2011) norms. Much less

support was found, however, for inclusion of other in self (see Stasiuk & Bilewitz, in press, for an exception). Furthermore, a number of new mediators were proposed including: outgroup trust (see Tausch et al., 2011), threats to the ingroup (e.g., Pettigrew et al., 2007), self-disclosure (e.g., Turner et al., 2007a), and ignorance about the outgroup (e.g., Eller et al., 2011; 2012). As no meta-analysis is available yet on the mediators of extended friendships, and since there are disproportionate numbers of studies looking into intergroup anxiety and norms as mediators, it cannot yet be said with certainty which mechanism or mechanisms are better at explaining the effects of extended friendships on prejudice than others.

Extended friendships – Pending Issues

Does the ‘direction of causation problem’ apply to extended friendships?

While establishing the direction of causation between contact and prejudice was regarded to be of utmost importance for direct contact and particularly friendships, the same has not been the case for extended friendships. Wright et al. (1997), in fact, claimed that the ‘direction of causation problem’ does not really concern extended contact as one cannot choose who one’s friends’ friends are (in other words the chances that the reverse path from prejudice to extended friendships is significant are rather low).

In real life, however, and particularly in contexts where contact is minimal (i.e., the contexts where extended friendships are meant to work best), it is quite likely that beliefs regarding intergroup relations are a very integral part of an individual’s values. To the extent that friendships are made based on similar values then the likelihood exists that a more tolerant individual has friends who are also tolerant and who might therefore have outgroup

friends. There is currently no study which sought to find whether the path from prejudice to extended friendships exists in a context where opportunities for contact are low.

Furthermore, as I noted earlier, causal claims are also made when mediation is proposed, as is the case for extended friendships. The vast majority of research on extended friendships to date has been cross-sectional (for exceptions, see e.g., Christ et al., 2010, Study 2; Feddes et al., 2009, which are described in detail in Chapter 5). More longitudinal studies are therefore needed in order to test for the reverse path and in order to establish the pathway via which extended friendships lead to prejudice reduction.

Are extended friendships sufficient for prejudice reduction?

The very promising results with regard to the effectiveness of extended friendships in reducing prejudice that accumulated over the last few years have led various researchers to express the opinion that extended friendships provide “a unique means of improving intergroup relations [...] and can be used in areas where opportunities for contact are restricted” (Christ et al., 2010, p. 1163). The question was also raised whether extended contact (friendships) per se can be sufficient in reducing prejudice (see Eller et al., 2012). It could be argued therefore that when opportunities for direct contact and therefore direct contact are limited or non-existent, non-direct forms of contact (in this case, extended friendships) provide *sustainable alternatives* to direct contact. I believe that the terms ‘alternative’ or ‘sufficient/ sustainable’ should be used cautiously while also defining exactly what one means by them.

One could say that B forms the *alternative* to A, if B, when used in the place of A, has the same effects as A. If we replace B with extended friendships and A with direct friendships, I would think that for extended friendships to be labeled as an alternative to direct friendships,

then the effects of extended friendships and the effects of direct friendships, if compared, should not be significantly different. In order also for extended friendships to be a *sustainable* alternative, the longevity of their prejudice-reduction effects needs to be both established as well as compared with the longitudinal effects of direct friendships.

There is only one study that attempted to *directly compare* the effects of direct and extended friendships to date, and that is the study of Feddes, Noack, and Rutland (2009) who assessed and compared the *longitudinal* effects of direct and extended friendships between German and Turkish school children on their outgroup evaluation. While Feddes et al. (2009) found that, cross-sectionally, both types of friendships were associated with more positive evaluations of the outgroup, this was not the case longitudinally. No temporal effects could be found for extended friendships as opposed to direct friendships.

As I will explain in more detail in Chapter 5, the study by Feddes et al. (2009) was conducted in a diverse context (mixed German schools), however, where the levels of intergroup contact between German and Turkish children were in any case rather high. A direct comparison of the longitudinal effects of direct and extended friendships on prejudice in contexts of low or no intergroup contact has yet to be attempted.

Do direct and extended friendships work via the same or different mechanisms?

As one may notice, some of the mechanisms proposed to explain the effects of extended friendships on prejudice are the same as the mechanisms suggested to explain the effects of direct friendships. Of these common mechanisms, intergroup anxiety was found to be the strongest mediator for direct contact and its role as a mediator of the effects of extended friendships on prejudice has been repeatedly supported. The mechanisms that are believed to

be unique for extended friendships are ingroup and outgroup norms.²⁹ So far research has produced mixed findings with regards to the exclusivity of norms as mediators of extended friendships³⁰ and therefore further research is necessary to shed light on the similarities or differences in the way the two types of friendships bring about their effects.

Beyond extended friendships: vicarious and imagined contact

While extended friendships have turned out to provide a promising way of improving intergroup relations in contexts where opportunities for contact are limited (see Christ et al., 2010), Crisp, Stathi, Turner, and Husnu (2008) pointed out that extended friendships presuppose at least *some* opportunities for contact. They say:

“[extended contact] cannot fully side step the opportunity for contact issue.

While one does not need to engage in contact oneself to reap the benefits, actual contact is still required somewhere in one’s wider social network (be it with one’s friend, family member, or just another ingrouper). If segregation defines the relationship between communities, one simply may not know of anyone who has anything to do with the outgroup” (p. 3).

Developing the idea of extended contact, researchers proposed other forms of non face-to-face contact as a solution to the *complete* lack of opportunities (or interest) for direct (or extended) contact. Crisp et al. (2008) did not make the distinction between extended friendships and vicarious contact when making their comments about extended contact requiring at least *some* opportunities for contact. In my opinion, their comment only applies to

²⁹ This is thought to be the case since group salience is needed, according to Wright et al. (1997), for inferences to be made about the norms of the ingroup and the outgroup. Since extended contact is thought to elicit higher group salience in comparison to direct contact, particularly direct friendships, ingroup and outgroup norms should be more obvious to individuals having extended as opposed to direct friendships.

³⁰ More on this will follow (see Chapter 5).

extended friendships where one needs to *know* someone who has a positive intergroup experience. Vicarious contact does not require knowing the ingrouper whose friendly intergroup encounter one is observing. I would therefore include vicarious contact in the list of indirect types of contact that can overcome the complete lack of opportunities issue.

Vicarious contact

Vicarious contact has so far been studied in three main ways. The first way is via laboratory contact interventions where the individuals (in the experimental group) observe a positive intergroup interaction either:

- i) in real-time: where the intergroup interaction is observed by the participants while it is actually happening (see for example Study 4 of Wright et al., 1997), or
- ii) not in real-time: where participants are presented with a video (e.g., a short clip) featuring the interaction between an ingrouper and an outgroup (e.g., Mazziotta, Mummendey, & Wright, 2011; West & Turner, 2014), or by showing participants extracts from movies and popular TV programs, that are otherwise widely accessed by the public, which portray cross-group friendships (e.g., Vittrup & Holden, 2011).

The second way is via field experiments where individuals (in the experimental group) get to observe (or listen to) interactions between the ingroup and the outgroup either:

- iii) via (ordinary) mass media (e.g., television or radio): Paluck (2009), for example, in a one-year long intervention in Rwanda, exposed the participants in the experimental group to a radio soap opera that contained messages about prejudice reduction and violence amelioration³¹.

³¹ The control group was exposed to a neutral-themed soap opera.

- iv) via other media (e.g., stories): Cameron and Rutland (2006) in a six-week long intervention had non-disabled children reading in small groups, along with an experimenter, stories featuring non-disabled and disabled children in friendship contexts.

The third way of studying vicarious contact is by measuring the everyday exposure of people to mass media picturing positive intergroup relations: e.g., Ortiz and Harwood (2005) measured their participants' exposure to TV series portraying gay-straight and Black-White relationships.

Effectiveness of vicarious contact

Observing a clip portraying a positive interaction between the ingroup and the outgroup was found to lead to improved outgroup evaluation of a rival basketball team (Gomez & Huici, 2008) and to more positive outgroup attitudes towards and increased willingness for contact with an isolated outgroup, the Chinese in Germany (Mazziotta et al., 2011). Castelli, Carraro, Pava, Murelli, and Carraro (2012, Study 1) found that participants who observed a live interaction between a White and a Black adult expressing friendly or unfriendly non-verbal behaviour influenced their implicit attitudes positively in the first case and negatively in the second. Furthermore, Castelli, De Dea, and Nesdale (2008) found that when young (3-7 year old) children observed a positive interaction between a Black and a White person, they evaluated the outgroup more positively than children who observed a negative interaction. Vittrup and Holden (2011) showed White children aged 5 to 7 years video extracts from popular TV shows which featured interracial friendships among characters to find that, in comparison to the control condition (no videos), the intervention led to improved attitudes towards Blacks.

In a very innovative recent study, West and Turner (2014) investigated the effects of vicarious contact on explicit outgroup attitudes and behavioral intentions as well as on physiological reactions prior to actual intergroup contact, non-verbal behaviour during actual intergroup contact, and reported positivity of actual intergroup contact. West and Turner (2014) had a naïve female participant interacting with a stranger who she was led to believe had schizophrenia. Participants in the experimental condition observed a clip of this interaction and they were made aware of the fact that the male figure in the interaction had schizophrenia whereas participants in the control condition who observed the same clip were not told that the male participant had schizophrenia. Results showed that the individuals in the experimental condition reported more positive attitudes towards people with schizophrenia and that this change of attitudes yielded more positive behavioural intentions towards the outgroup. They also found that participants in the vicarious contact (experimental) condition displayed smaller anticipatory stress responses prior to the actual interaction in comparison to participants in the control condition and their interaction with the outgroup was judged to be more positive by the ‘outgroup’ (who was a confederate in reality). Furthermore, independent judges recorded more positive non-verbal behaviours during the actual interaction by the individuals who were placed in the experimental condition in comparison to participants in the control condition.

As for more long-term interventions, story-reading was found to lead to: greater tolerance for immigrant groups amongst Finnish adolescents who attended schools with a high density of immigrants (Liebkind & McAlister, 1999), more positive attitudes towards disabled children by non-disabled children (Cameron & Rutland, 2006), more positive attitudes towards refugees for school children in England (Cameron, Rutland, Brown, & Douch, 2006),

and better attitudes towards an ethnic minority (Indians) for ethnic majority (English) children (Cameron et al., 2011). Importantly, the effects of vicarious contact (long-term) interventions were positive even in contexts of deep-rooted conflict such as Rwanda, where Paluck (2009) found that the intergroup-related (in comparison to the health-related) soap opera changed listeners perceptions of social norms as well as their behavioural dispositions with regards to a number of sensitive intergroup issues like trust, cooperation, and trauma healing.

Regarding vicarious contact through real-life exposure to mass media, Ortiz and Harwood (2007) found that exposure to two TV shows - one depicting positive contact between homosexuals and heterosexuals and one depicting positive contact between Blacks and Whites - was associated with more willingness to approach the outgroup (i.e., less social distance). The association was significant even after controlling for total media exposure and prior contact. Similar results were found by Schiappa, Gregg, and Hewes (2005), who showed a negative relationship between the exposure to a gay-friendly film and prejudice against gay men. The TV program that really stands out in terms of the frequency with which it was examined in the relevant research is *Sesame street*. Being exposed to the program was found to promote social tolerance (Lovelace, Scheiner, Dollberg, Segui, & Black, 1994; Tidhar & Schacter, 1986) and more positive attitudes towards African and Latino Americans by European American children (Bogatz & Ball, 1971). Interestingly, exposure of Israeli, Palestinian-Israeli, and Palestinian preschoolers to an adapted version of *Sesame Street* in Israel and Palestine was found to yield positive pretest-posttest changes for a series of intergroup outcomes including the type of attributes the participants used to describe the outgroup (Cole, Arafat, Tidhar, Tafesh, Fox, Killen, & Yung, 2003).³²

³² Note, however, that in Cole et al.'s (2003) study no control condition was used.

Imagined contact

Imagined contact, “the mental simulation of a social interaction with a member or members of an outgroup category” (Crisp & Turner, 2009), was first proposed and tested by Turner, Crisp, and Lambert (2007c). Turner et al. (2007c) found that young participants who imagined a positive intergroup interaction with an elderly individual exhibited less intergroup bias (Experiments 1 and 2) and that heterosexual men who imagined a positive interaction with a homosexual man evaluated the outgroup more positively and perceived the outgroup to be more variable (Experiment 3). Turner et al. (2007c) carefully selected their control conditions in order to rule out alternative explanations for the effects of the imagined contact intervention. They tested their main experimental scenario where participants were asked to imagine a positive interaction with the outgroup against: a) a positive scenario where participants had to imagine a pleasant (Experiment 1) or an unexpected (Experiment 3) scene, to control for the induction of positive affect and cognitive load respectively, and b) to a scenario in which participants had to simply think of the outgroup, to control for any effects that mere priming of the outgroup had on the results (Experiment 2). Regardless of the control condition used (positive/ unexpected scene or outgroup priming), imagined contact led to positive intergroup outcomes.

Furthermore, Turner et al. (2007c) tried to assess whether task demand characteristics affected the participants’ responses. Turner et al. (2007c) asked the participants in their three studies at the debriefing session following the experiment whether they had worked out what the experiment was after. Only four out of the 79 participants said they had figured out what the experiment was about but their guesses were incorrect. A subsequent study by Turner and Crisp (2009) also found imagined contact to be effective not only for the explicit attitudes,

which are prone to task demand characteristics, but also for implicit attitudes which are much less likely to be affected by demand characteristics.

Imagined contact's effectiveness

The meticulous control for alternative explanations of imagined contact increased confidence in the capacity of the mental simulation of a positive intergroup encounter to yield positive intergroup outcomes. The imagined contact studies, following Turner et al.'s (2007c) imagined contact hypothesis, gave further support for imagined contact's effectiveness in eliciting less prejudice and more positive evaluations for a number of outgroups including: a) rival ethnic groups: Greek Cypriots (Husnu & Crisp, 2010b), b) ethnic minorities: e.g., Muslims in Britain (Husnu & Crisp 2010 a,b; Stathi, Crisp, & Hogg, 2011; Turner & Crisp, 2009), indigenous people in Mexico (Stathi & Crisp, 2008, Experiment 1), c) people with a mental illness (West, Holmes, & Hewstone, 2011), d) older adults (Turner et al., 2007c; Turner & Crisp, 2009), e) homosexuals (e.g., Turner et al., 2007c, Experiment 3) and e) socially estranged outgroups: Roma in Germany (Kuchenbrandt, Eyssel, & Seidel, 2013), illegal immigrants in the U.S. (Harwood, Paolini, Joyce, Rubin, & Arroyo, 2011), asylum seekers in England (Turner, West, & Christie, 2013, Experiment 1), and immigrants in Italy (Vezzali, Capozza, Stathi, Giovannini, 2012).

Imagining positive and intergroup contact: necessary and sufficient conditions for imagined contact

All the (successful) imagined contact scenarios incorporated the two conditions that were identified by Crisp and Turner (2009, see also Crisp, Stathi, Turner, & Husnu, 2008) to render imagined contact effective in bringing about positive outcomes. These two conditions, are that the imagined scenario has to: a) involve the simulation of an intergroup interaction, as

opposed to either merely thinking about the outgroup (see Turner et al., 2007c, Experiment 2) or imagining a positive interaction with an unspecified stranger (Turner et al., 2013) and b) have a positive tone, as opposed to a neutral one (e.g., Stathi & Crisp, 2008, Experiment 1; West et al., 2011, Experiment 1).

Crisp and Turner (2009), moreover, maintained that while these two conditions are both sufficient and necessary for imagined contact to be successful, there may be other facilitating conditions that will lead to specific outcomes or to the enhancement of the intervention's effects. An example of such a facilitating condition has been identified and tested by Husnu and Crisp (2010 a, b) who supplemented the basic positive intergroup scenario with information aimed at encouraging participants to engage in more elaborated and vivid imagery. They hypothesized (and found) that this would heighten the participants' intentions to engage in contact with the outgroup.

Furthermore, Stathi et al. (2011), drawing on the Mutual Ingroup Differentiation Model (Brown & Hewstone, 2005), added to the positive imagined contact scenario information about the typicality of the outgroup member (Experiment 3) and the group's salience (Experiment 5) to find that when the scenario portrayed an outgroup member who was a typical versus atypical member of the outgroup, and when group salience was high rather than low, participants reported heightened self-efficacy for intergroup contact. None of these two amendments (typicality and group salience) was tested against the 'standard' imagined contact condition though and no control condition was used either.

Extending the findings of Stathi et al. (2011), Pagotto, Visintin, De Iorio, and Voci (2013) found that after initially introducing intimacy (via self-disclosure) in the imagined interaction between an individual (non-Muslim Italian) and the imagined outgroup member (Muslim

stranger living in Italy), the imagined contact scenario that addressed intergroup differences³³ led to (marginally) more favourable outgroup attitudes in comparison to a second imagined contact scenario which retained an interpersonal character by asking participants to talk about their holidays, hobbies, favourite books, TV programs etc, as well as a no-contact control condition. Attitudes in the interpersonal imagined contact were not different from the control condition. They also found that the intergroup condition was significantly different from the control condition in terms of willingness to cooperate with the outgroup, unlike the interpersonal scenario which was not significantly different from the control condition. It should also be underlined here, that the manipulation checks utilized by Pagotto et al. (2013) showed that the two imagined contact conditions did indeed differ from each other in terms of the extent to which the focus of the participants was on the interpersonal vs. intergroup elements of the interaction. The two conditions, however, did not differ in terms of positivity which strengthened Crisp et al.'s (2009) contention that imagining an intergroup (as opposed to interpersonal) and positive interaction is both necessary and sufficient for imagined contact to yield positive effects.

Imagined contact as a preparation for actual/direct contact

In a recent theoretical review of imagined contact, Crisp and Turner (2013) stressed that one of the primary functions of imagined contact is as a pre-contact tool (see also, Crisp, Husnu, Meleady, Stathi, & Turner, 2010). They argued that imagined contact can prepare individuals for more intimate types of contact, like direct contact, by: a) instigating interest for actual contact as well as creating positive behavioural dispositions towards the outgroup and

³³ In this scenario participants were asked to imagine that while discussing with the Muslim individual they expressed their wish to maintain their own values, religious, and cultural traditions, whereas the Muslim outgroup member explains to the individuals that his/her own traditions and religious norms are of personal importance to her/him and therefore she/ he does not wish to give to them up.

b) psychologically preparing them for the contact situation itself (e.g., via reducing their anxiety for a prospective intergroup interaction)³⁴.

There is by now an impressive range of study findings that support this contention of Crisp and Turner (2013). In terms of increasing individuals' willingness and intentions for actual contact, imagined contact was found to increase the interest as well as the willingness to engage in a conversation with a Greek Cypriot for Turkish Cypriot students (Husnu & Crisp, 2010a, Experiment 1), to lead to more positive behavioural intentions towards British Muslims for non-British Muslims (Husnu & Crisp, 2010b, Experiment 1), and to yield a greater tendency for approach behaviours (e.g., talk to and spend time with the outgroup) towards asylum seekers for British high school students (Turner et al., 2013, Experiment 1).

The effects of imagined contact on behavioural intentions and approach tendencies were mediated by intergroup anxiety, thus showing that imagined contact does reduce anxiety for future intergroup encounters and also that through its influence on anxiety it can also lead to more positive behavioural intentions towards the respective outgroups. Furthermore, the study by Stathi et al., (2011) found support for imagined contact's influence on another variable that is related to the psychological preparedness for engaging in an actual interaction, namely contact-related self-efficacy. Participants who imagined a positive intergroup contact scenario reported believing they would feel more contact self-efficacy than people in the control condition (Experiment 1). Stathi et al. (2011) did not, however, proceed to check whether self-efficacy, like intergroup anxiety, mediates the effects of imagined contact on willingness for future contact.

³⁴ As discussed already, lower levels of contact breed higher anxiety (Plant & Devine, 2003; Stephan & Stephan, 1985). Furthermore intergroup anxiety is toxic when present in intergroup relations (see e.g., Richeson & Shelton, 2003). So individuals will be readier to reap the benefits of a positive interaction if they enter it with less anxiety.

Contact self-efficacy is a variable that is very relevant to intergroup research, but has only recently entered the contact field. Often, the reason for which individuals do not perform a behaviour is that they do not feel confident to (see Ajzen, 1991; Bandura, 1997). The same should be true for intergroup contact: what may stop people from having contact is not their impression/ evaluation of the outgroup but strong feelings of anxiety and inefficacy instead. The research findings discussed suggest, however, that such feelings can be ameliorated or changed when individuals are first exposed to the group indirectly.

Vicarious and imagined contact - Pending issues

Does vicarious contact prepare individuals for actual contact?

When opportunities for direct contact are non-existent (e.g., in conflict-torn and segregated contexts) or in those cases where contact is possible but for one reason or the other is not pursued (see Crisp et al., 2010; Crisp & Turner, 2013), imagined contact was proposed (and found) to provide both a gateway to the benefits of contact (as well as a preparatory step for actual contact. What this means in practice is that imagined contact can: a) enable individuals to, as Crisp and Turner (2013) put it, “access the benefits of contact” (p. 135) (i.e., influence the way one sees, thinks, and feels about the outgroup) and b) orientate individuals towards actual contact both by preparing them for the actual interaction (e.g., via reducing anxiety) and by rendering them more willing to pursue contact or approach the outgroup when the opportunity emerges.

Extended contact was also proposed as a stepping stone to direct contact (see Eller et al., 2012; Gomez et al., 2011), but little reference has been made to vicarious contact (as an instance of extended contact) and its ability to prepare individuals for direct contact. Although

there are some studies showing that vicarious contact leads to greater preparedness for actual contact (see Mazziotta et al., 2011³⁵; Ortiz & Hawrwood, 2007; Paluck, 2009), the support vicarious contact has derived for its ability to render individuals ready and interested for a prospective face-to-face interaction with the outgroup is meagre (for an exception see West & Turner, 2014), especially when compared to the support accumulated for the other major type of indirect contact: imagined contact). This is rather an odd development particularly because contact researchers (see for example Mazziotta et al., 2011; Turner et al., 2007b), drawing from the socio-cognitive learning theory (see Bandura, 1965; 1986; 1997; 2004), sought to justify why the observation of a positive intergroup interaction, just like the observation of any modeled behaviour, will lead the observer to learn from and possibly adopt the modeled behaviour. Vicarious contact, just like imagined contact, should therefore be able to both psychologically prepare individuals for a prospective interaction and to render them more willing to engage in it. These contentions have not yet received adequate support and further research is needed to verify whether vicarious contact can indeed bring about not only a positive evaluation of the outgroup but also greater readiness for actual contact.

Comparison of direct, vicarious, and imagined contact

Imagined contact researchers (e.g., Crisp et al., 2009; Crisp & Turner, 2009) advocated that imagined contact is not a replacement for other types of contact, i.e., direct *as well as* extended contact, the effects of which, at least with regards to attitude change, they forecasted to be stronger than the effects of imagined contact³⁶. Rather confusingly, however, in some cases, imagined contact was presented as the type of indirect contact that is most similar to

³⁵ Mazziotta et al.'s (2011) study tested the effects of vicarious contact on behavioural intentions as well as the mediation of this effect by self-efficacy.

³⁶ Whenever Crisp and colleagues referred to extended contact they did not distinguish between extended friendships and vicarious contact, so it is not clear if they refer to both.

direct contact as the self in both imagined and direct contact partakes in the intergroup interaction (see Crisp & Husnu, 2011). Because of that, imagined contact was claimed to be able to produce effects that are similar to direct contact (see Turner et al., 2013). Despite these claims, a formal comparison between imagined and direct contact or between imagined contact and other types of indirect contact, like vicarious contact, has never been conducted.

Crisp and Turner (2013) recently suggested that imagined contact as a form of indirect contact has much in common with extended contact, and suggested that the two forms of contact should be *directly compared*. According to Crisp and Turner (2013), a direct comparison of the two types of contact will help delineate their similarities and differences, something that will aid practitioners to customize their interventions in different contexts. I believe that a very useful comparison is the one between imagined and vicarious contact in terms of their relative effectiveness to promote a better evaluation of the outgroup but also to prepare individuals for actual contact. I deem this comparison as important, particularly because both of these contact types were suggested (and to a great extent found) to be successful in achieving these end goals³⁷.

Lastly, whereas Wright et al. (1997) outlined the potential benefits that extended contact can have over direct contact (namely preservation of group salience while in an anxiety-free setting), a comparison of the effectiveness of the two is expected to favour direct contact, particularly with regards to outgroup attitudes (see Turner et al., 2007b).³⁸ While there is some research comparing direct and extended contact (friendships) (e.g., Feddes et al.,

³⁷ Even though I chose to focus on the comparison of imagined and vicarious contact in this thesis, the comparisons between imagined and extended or imagined, vicarious, and extended contact are also of theoretical interest.

³⁸ This argument is based on the literature on attitude formation, and the findings according to which attitudes formed via direct exposure to the attitude object are likely to be stronger than attitudes formed via indirect experiences to the attitude object (see Fazio & Zanna, 1981, for a review).

2009; Paolini et al., 2004) there is no research to date comparing vicarious with face-to-face contact.

Assessing contact interventions

The assessment of interventions within the intergroup contact literature has almost exclusively focused on the breadth (i.e., how many dependent variables the intervention has an effect on) and the magnitude of the effects (how strong these effects are). The focus is also on the temporary effects, i.e., the effects as captured immediately after either direct contact (e.g., Al Ramiah & Hewstone, 2012) or indirect (extended) contact (e.g., Eller, Abrams, & Zimmermann, 2011) intervention. A neglected element of the evaluation of contact-based intergroup interventions pertains to the longevity of the interventions' effects. There is no vicarious contact study, to my knowledge, that has investigated the longevity of the effects of these two types of contact whereas there are only two imagined contact studies which measured the effects of a 3-week imagined contact intervention a week after the last contact session (see Vezzali et al., 2012; Vezzali, Capozza, Giovannini, & Stathi, 2012). If imagined and vicarious contact are to make individuals more open to actual contact though, their effects should have at least some duration up until the opportunity for contact actually emerges. I propose, therefore, that the evaluation of contact-based interventions (including vicarious and imagined contact) should incorporate the dimension of the longevity of these interventions' effects other than their breadth and magnitude. As Stephan and Stephan (2001) suggested, also, when evaluating an intervention one should, ideally, test participants prior to the intervention (pretest), immediately after the intervention (-immediate- posttest), and once again after some time has elapsed (delayed posttest) so as to assess whether the intervention has an impact that lasts in time.

Are 'positive' and 'intergroup' sufficient conditions for imagined contact to produce favourable outcomes?

Imagining an interaction that is intergroup and positive in nature has been seen to be adequate to produce favourable outgroup outcomes (see Crisp et al., 2008). Crisp and Turner (2009) noted that any other conditions will be facilitating and not necessary. This premise, however, has been recently contested by the results of a study by Kuchenbrandt et al. (2013). In this study, German participants imagined contact with a Roma individual that was either neutral, or positive, or positive *and* cooperative. Their results showed that both positive and positive-cooperative contact led to higher empathy and lower subtle prejudice than the neutral contact condition (with cooperative-positive contact leading to even higher empathy and even lower subtle prejudice). However, positive contact was not different from neutral contact on the measures of intergroup anxiety and trust, unlike positive-cooperative contact which yielded lower anxiety and higher trust in comparison to the other two conditions.

Given the emphasis imagined contact researchers placed on imagined contact rendering individuals psychologically ready for future contact, null-effects or negative effects on intergroup anxiety are troublesome. It seems that there is a possibility that in certain settings, or for certain outgroups, positive intergroup contact may not be adequate to yield a comprehensive change in intergroup outcomes. While positive intergroup contact may be efficient in leading to positive outcomes for the majority of the outgroups tested, imagined contact with some outgroups may need additional conditions to be fulfilled (e.g., cooperativeness) in order to be effective.

Intergroup contact in Cyprus

With the partial lifting of the restrictions of mobility vis-à-vis the Green Line in 2003, opportunities for intergroup contact between the two communities in Cyprus were reinstated. While multiple crossings at the checkpoints can be considered as an indicator of bi-communal contact³⁹, the reality is that meaningful intergroup contact has been minimal in Cyprus (Johnson, 2007). Interestingly, intergroup contact has been particularly limited among young people. Sitas, Latif, and Loizou (2007) conducted a large study with a representative sample from the Greek Cypriot (N = 1000) and the Turkish Cypriot (N = 1000) communities. Their study looked into a number of variables that could potentially influence dispositions towards reconciliation. They categorized these variables into ‘hard variables’ (e.g., gender, age, and refugeeedom), and ‘soft variables’ (e.g., education, civic involvement, and intergroup contact). They grouped age into two categories, the younger group aged approximately 25 years and the older group, aged approximately 50 years. The basic difference between these two age groups in Cyprus is that the older group would have experienced the bi-communal conflict first-hand and they would also have also experienced pre-segregation contact with the other community. The younger generation on the other hand were born (and raised) in a divided island and had only had a second-hand experience of the conflict, and very little, or no, contact.

Sitas et al. (2007) found that a greater percentage of the older cohort (31 %) reported having a strong exposure to the other side (North/ South) in comparison to the younger cohort (12%), and a greater percentage of the older cohort reported multiple encounters with people from the other side (34% as opposed to 18%). Furthermore 50% of the young cohort reported

³⁹ According to the latest annual report of the European Commission to the European Council dated 26.5.2013, however, there had been a decrease in the numbers of Greek and Turkish Cypriots crossing the line in 2012 in comparison to the previous years. The number of crossings has been gradually decreasing in the last three years according to the corresponding annual reports.

no encounters with the other community as opposed to 37% of the older generation. With regards to crossings, more young people (31%) than older people (19%) reported not having crossed over to the other side at all.

Within the Greek Cypriot community which forms the focus of this dissertation, contact has been scarce amongst the youth and there have been no signs of this changing throughout the years. A study amongst Greek Cypriot university and college students in Nicosia (N = 390) in 2006 revealed that 80% of the participants reported having no Turkish Cypriot friends, and 10% reported having one or two outgroup friends. Also, 36% of them reported having never crossed over to the other side (Psaltis and Pachoulides, personal communication). A study amongst university students in 2009 yielded similar figures: 81% of the respondents reported having no Turkish Cypriot friends and 14% reported having one or two friends. Furthermore, 31% of the participants said they had never crossed to go to the other side, 41 % of them reported having *never met* a Turkish Cypriot (while 26% said they met one or two Turkish Cypriots in their lives) (Kyprianou, 2009).

Despite the scarcity of contact, particularly amongst the youth, the little research on the effects of intergroup contact in Cyprus has yielded promising results concerning intergroup relations. Webster (2005), for example, using a sample of 1000 Greek Cypriots found that contact with Turkish Cypriots fostered more optimistic views about the prospects of intergroup interaction. He also found that this effect of contact on individuals' optimism was not moderated by any demographic variables including age. Sitas et al. (2007) furthermore found contact to be one of the strongest determinants of pro-reconciliation dispositions. According to the study's results, 75% of the individuals with the strongest (positive) dispositions towards reconciliation were those people who claimed to have not only

been exposed to the other side but those who also reported having had “strong and intense encounters with people there” (p. 35).

Even though there have been many externally-sponsored bi-communal camps hosted both in Cyprus and abroad aimed at the youth of the two communities (see for example Ungerleider, 2001, and Loizos, 2006, for a review), formal quantitative or qualitative assessments of the outcomes of these camps are very rare. As an exception to this, Lyras (2007), conducted a six-day long structured intervention with 96 Greek Cypriot and Turkish Cypriot school children (13 – 16 years old), and proceeded to assess its outcomes. Children in the camp interacted playing sports, engaging in team activities that fostered teamwork, and engaged also in daily conflict management activities. Lyras (2007), adopting a pretest - posttest design, found positive pre-to-posttest changes with regards to children’s attitudes towards cross-cultural interaction, friendship, and collaboration. Moreover, the majority of participants (77 %) reported at posttest that they had perceived a ‘significant positive’ or ‘some positive’ change in their attitudes towards the other community whereas only 5 % of them reported having experienced ‘some negative change’. Results did not differ for the two groups.

Tausch, Hewstone, Kenworthy, Psaltis, Schmid, Popan, Cairns, and Hughes (2010) assessing the generalised effects of intergroup contact in Cyprus on secondary outgroups across three different contexts, found that contact between Greek Cypriots and Turkish Cypriots predicted more favourable outgroup attitudes towards the primary outgroup (i.e., Turkish Cypriots and Greek Cypriots respectively) as well as the secondary outgroup: Greeks (for Turkish Cypriots) and Turks (for Greek Cypriots). The indirect effect of contact with the

primary outgroup on attitudes towards the secondary outgroup was significantly mediated by attitudes towards the primary outgroup for both communities.

Despite the relative lack of direct contact in Cyprus, research on other forms of contact has been almost non-existent. The only exception to that is the study by Husnu and Crisp (2010a) who asked Turkish Cypriot students to imagine a positive intergroup interaction with a Greek Cypriot stranger. Husnu and Crisp (2010a) found that the participants who imagined a positive intergroup interaction reported greater intentions towards future contact in comparison to those participants who imagined a non-contact scene.

In summary, opportunities for contact have recently emerged in the highly segregated island of Cyprus. Despite this, the number of people having contact especially amongst the youth is limited. Those people who had intergroup contact either in the form of every day naturally occurring contact (e.g., Tausch et al., 2010; Webster et al., 2005) or in the form of interventions (see Husnu & Crisp, 2010a; Lyras, 2007) report more optimism about intergroup relations (Webster, 2005), more positive attitudes towards the outgroup (Lyras, 2007) as well as a secondary (Tausch et al., 2010) outgroup, more willingness for contact (Husnu & Crisp, 2010a), and more positive dispositions towards reconciliation (Sitas et al., 2007). Very little is known about the effects of indirect types of contact in the Cypriot context. Given the absence of direct contact (particularly amongst Greek Cypriot youth) the study of indirect types of contact and their comparison with direct contact is necessary and potentially very useful.

Conclusion and present research

Research exploring Allport's (1954) hypothesis that intergroup contact will under certain conditions lead to prejudice reduction, has shown, that contact will bring about more positive affective and cognitive outcomes particularly when contact is of good quality and

even more so when it provides the opportunity for the individuals to become friends. The effects of contact are also more likely to generalise from the specific outgroup to the outgroup as a whole (as well as to other outgroups) if contact preserves its intergroup character, i.e., when it is perceived to be an event between two people coming from different groups (see Brown & Hewstone, 2005, for a review). Furthermore, research on intergroup contact has also delineated the ways via which contact reduces prejudice. A relevant meta-analysis yielded that affective mediators, and particularly intergroup anxiety, best explain the association between contact and prejudice (Pettigrew & Tropp, 2008).

While extensive support was given to the existence of a negative association between contact and prejudice, which was consolidated via meta-analyses (see Davies et al., 2011; Pettigrew & Tropp, 2006), less emphasis has been given to whether this association is better accounted for by the fact that contact reduces prejudice or by the fact that less prejudiced people have more contact. To date, a number of studies have set out to address the ‘direction of causation problem’ (see Pettigrew, 1998). Nonetheless, little has been done in order to provide support to the proposed chain of causal relationships going from contact to mediators and from mediators to prejudice as they develop in time. Instead, most research studying the way/s through which contact leads to prejudice have relied on cross-sectional data.

Indirect forms of contact have been proposed in order to provide an alternative route to prejudice reduction, particularly when direct contact is limited (Eller et al., 2012; Turner et al., 2007b). Of these indirect forms of contact, extended friendships (Wright et al., 1997) - knowing an ingrouper who has an outgroup friend - have received the greatest attention and support. Extended, like direct friendships, were found to be negatively associated with outgroup prejudice, and this association seems to be explained by both anxiety reduction as

well as by a set of other mechanisms (e.g., ingroup and outgroup norms) (see Turner et al., 2008). Social norms, in particular, are thought to be unique mediators of extended (as opposed to direct) friendships' effects on prejudice (see Wright et al., 1997).

Extended friendships were proposed to be alternatives to direct friendships and even sufficient in terms of bringing about prejudice reduction (see Eller et al., 2012). There is, however, little support for this contention. Direct and extended friendships have rarely been compared especially in terms of their longitudinal effects. There is also very little research comparing the mechanisms driving the effects of these two types of contact.

Finally, vicarious and imagined contact can be seen as alternatives to both direct contact as well as extended friendships, when the either type of friendships is not feasible. It could be proposed that the ultimate usefulness of these two types of indirect contact lies in that they can prepare individuals for actual contact by increasing their interest and willingness for it and also by rendering them psychologically more ready to enter an interaction with an outgroup member. While support was given for imagined contact's ability to yield readiness and willingness for actual contact (see Crisp & Turner, 2013), there is less support for vicarious contact's capacity to prepare individuals for face-to-face contact. There is also no study so far comparing these two types of contact with each other or each of these two types of contact with direct contact despite the recognition that these comparisons need to be made (e.g., Crisp & Husnu, 2011; Crisp & Turner, 2013).

Lastly, while imagining positive contact turned out to be adequate to bring about positive intergroup outcomes in most cases, a recent study by Kuchenbrandt et al. (2013) showed that positive contact alone did not yield any differences in anxiety in comparison to neutral contact with Roma people. These findings raise the question of whether in some

contexts, or for certain outgroups, imagining positive intergroup contact may not be enough for yielding desired outgroup outcomes (such as anxiety reduction).

This dissertation comprises six studies, five experimental and one longitudinal study which are grouped into three sections, described below. All studies were conducted in Cyprus, a setting where direct contact has been remarkably limited even after opportunities for contact were reinstated. All participants were Greek Cypriots living in Cyprus and Turkish Cypriots formed the outgroup in all studies. The experimental studies utilized predominantly female undergraduate students attending academic institutions in the capital of Cyprus, Nicosia. The longitudinal study utilized a representative sample of the Greek Cypriot adult population.

The first section (see Chapter 3) consists of a series of three experiments which aimed to experimentally *assess* as well as *compare* the effectiveness of direct (face-to-face) and indirect (vicarious and imagined) types of contact in bringing about more positive outgroup attitudes (Experiments 1-3), less intergroup anxiety for future contact (Experiments 2 and 3), and more positive action tendencies towards the outgroup (Experiment 3). All experiments used the same basic pretest-intervention-posttest design, with a delayed posttest added in Experiments 2 and 3. Apart from the dependent measures (attitudes, anxiety for future contact, and action tendencies), items measuring how participants perceived the interaction were also included.

Experiment 1 assessed and compared the effects of direct and vicarious contact in terms of yielding more positive outgroup attitudes. It also tested Wright et al.'s (1997) conjectures that extended contact (here, vicarious contact) is less-anxiety provoking than direct contact, and that the interaction is more likely to be perceived as an 'intergroup'

experience when observed (vicarious contact) in comparison to when it is experienced (direct contact).

Experiment 2 sought to replicate Experiment 1 and to extend it by adding one more measurement point (a delayed posttest) as well as a no-contact control condition in order to assess and compare the sustainability of direct and vicarious contact's effects on outgroup attitudes as well as anxiety for future contact both in comparison to their pretest scores as well as in comparison to the control condition.

Experiment 3 assessed and compared the effects of direct contact, vicarious contact, imagined contact, and a control condition (no contact) in yielding pretest-posttest changes in outgroup attitudes, anxiety for future contact, and action tendencies. It also investigated whether these effects endured in time, by including a delayed posttest. The three types of contact were also compared on other measures tapping various aspects of the intergroup interaction (e.g., how much anxiety each contact type provoked and how salient the two group categories were during the interaction).

The second section (see Chapter 4) is made up of two imagined contact studies, both experimental. Experiment 4 explored the possibility of incorporating into the 'standard' positive imagined contact scenario (see Crisp et al., 2008; 2009) information about the similarities, differences, or the similarities and differences between the self and the outgroup as well as their respective groups. The hypothesis was that the scenario balancing similarities and differences would yield the most positive attitudes towards the outgroup as opposed to both the scenario that stressed similarities and the scenario that stressed differences between the ingroup(er) and the outgroup(er). A second hypothesis was that the difference on the dependent variable (outgroup attitudes) between the scenario incorporating similarities and

differences and the similarities-only scenario would be attributed to the similarities-only scenario causing distinctiveness threat.

Experiment 5 tested the imagined contact scenario that yielded the most favourable outgroup attitudes in Experiment 4 against the ‘standard’ positive imagined contact scenario and against a control condition, in terms of their effects on both outgroup attitudes as well as on a number of additional dependent variables (intergroup anxiety, contact self-efficacy, and action tendencies). The hypothesis was that the scenario entailing more information about the outgroup would better prepare the individuals for an actual interaction (due to reduced intergroup anxiety and increased contact self-efficacy), and that it would lead to more positive action tendencies in comparison to the control condition as well as the ‘standard’ imagined contact condition.

Section 3 (see Chapter 5) contains one three-wave longitudinal study (Study 6) with a time-lag of six months between measurements. Study 6 aimed to: a) establish the direction of causation between direct and extended friendships and outgroup attitudes, b) assess *and* compare the longitudinal effects of direct and extended friendships on outgroup attitudes, c) assess whether the effects of direct and extended friendships on attitudes are mediated either by intergroup anxiety or by ingroup norms, or both, and d) compare the relative strength of the indirect paths within and between direct and extended friendships to assess whether there are differences in the mechanisms explaining their respective effects on attitudes.

Chapter 3: Comparing the effects of direct and indirect types of contact experimentally

Since Wright et al.'s (1997) seminal work on extended contact, the literature on indirect forms of contact has grown. As mentioned earlier, there are by now a number of studies providing support to the extended contact hypothesis, including support for extended friendships (e.g., Turner et al., 2007a) and vicarious contact (e.g., Mazziotta et al., 2011; West & Turner, 2014). In the last five years extensive support was also found for the prejudice-reduction effects of another subtype of indirect contact, imagined contact (see Turner et al., 2007c). Indirect types of contact have been predominantly conceived as alternatives to direct contact for reducing prejudice, especially in settings where actual contact is not or had not been an option. Of all indirect contact types, extended friendships are the most studied. Extended friendships' contribution to prejudice reduction was found to be particularly profound in low-contact contexts. In these contexts, the effects of extended friendships on prejudice were found to go above and beyond the effects of direct friendships (e.g., Christ et al., 2010), thus rendering extended friendships to be a promising alternative of direct contact when that is absent.

Unfortunately, however, as Dixon et al. (2008) correctly pointed out, extended friendships, like direct friendships, are not common occurrences. In settings where direct contact is low either because it is unfeasible or because it is not pursued, the other subtype of extended contact, namely vicarious contact, and also imagined contact, are better seen as interventions that can lead to at least temporary outcomes that are comparable to the outcomes of direct contact (in terms of improving intergroup relations) and that can prepare participants for actual contact. The potential contribution of indirect contact interventions in preparing participants for future contact, has been explored within the imagined contact paradigm (e.g.,

Husnu & Crisp 2010 a, b; Turner & West, 2012) but not to the same extent by vicarious contact studies (for an exception see West & Turner, 2014). As regarding the comparability of the effects of indirect contact interventions and the effects of direct contact interventions, this was never explicitly addressed in the past. The first general question being dealt with in this chapter, therefore, is whether indirect types of contact can bring about effects that are analogous to those of direct contact.

A second research question being addressed in these studies pertains to the longevity of the effects of indirect in comparison to direct contact interventions. To my knowledge, previous studies testing the effects of indirect contact interventions on a number of prejudice-related measures solely focused on providing support to the short-term (i.e., immediate) effects each intervention had on the outcome measures (for an exception see Vezzali et al., 2012; 2012). Furthermore, no follow-up of the initial effects took place in any of the interventions falling under the general category of extended contact either⁴⁰.

One may wonder how useful a follow-up of these effects is, particularly since indirect contact interventions were never understood to be permanent alternatives to direct contact (Crisp & Turner, 2009), and so one should not expect their effects to be necessarily lasting. Additionally, the usefulness of interventions based on indirect forms of contact was thought to lie in that they can render individuals less apprehensive about and more willing to pursue future interactions. Yet, I deem follow-up tests as important for two main reasons. The first concerns the implications entailed in establishing the longevity of the effects of indirect types of contact. If indeed interventions based on indirect contact are consistently found to reduce

⁴⁰ It should be noted here that my reference to indirect contact interventions only incorporates one-off laboratory interventions and not field experiments where more long-term and full-scale interventions are employed. An example of the later type of interventions is the year-long intervention through radio soap operas conducted by Paluck (2009) in Rwanda.

individuals' intergroup anxiety and increase their willingness for contact, this would imply that should the opportunity for intergroup contact emerge, these individuals would be in a better place to seize it and thus enter a (possibly) positive face-to-face intergroup encounter. However, given the scarcity of contact in certain contexts, the opportunity for intergroup interactions is highly unlikely to come up at once, and in the case that it does arise, this is more likely to happen later in time. Consequently, if the positive effects of these interventions are shown to last at least for a certain period of time, this will work to strengthen the hypothesis that indirect contact interventions can prepare individuals for a future intergroup interaction.

Typically, the effects of an intervention are more profound when measured immediately after the intervention. When the effects are tested again after a certain amount of time elapses, the most common thing to observe is a level-off or a weakening of the initial strength of the effects (e.g., Weintraub & Johnson, 1997; and see also Stephan & Stephan, 2001, for a relevant discussion). Sometimes though, the follow-up measure can yield surprising results whereby, for example, delayed effects emerge while immediate effects never occurred. Therefore, a second reason for including a follow-up test is that it provides the researcher with the opportunity of checking for phenomena akin to the 'sleeper effect' phenomenon that was first described in persuasion studies (see Eagly & Chaiken, 1993, for a review). According to the 'sleeper effect' phenomenon, a delayed increase in the effects of persuasion is observed when a cue that was initially discounted later on becomes available in the memory of the recipient. In the context of the contact-intervention research, a delayed onset of effects could be the case, I would argue, especially for indirect types of contact where

the individuals who are not exposed to a direct experience may take their time to reflect on how the vicarious experience resonates with them.

The potential success of indirect types of contact in bringing about positive intergroup outcomes was attributed by Wright et al. (1997) to two main advantages indirect contact has in comparison to direct contact. The first advantage regards the fact that when one observes an intergroup interaction as opposed to being actively involved in it, one becomes more aware of the group memberships of the interacting individuals. In other words, groups are more salient for the individual observing an intergroup interaction than to the individual participating in it. The other advantage of indirect contact over direct contact identified by Wright et al. (1997) is that the indirect contact triggers less anxiety than direct contact. Wright et al. (1997) more specifically contended that: “[...] observing an in-group-out-group friendship involving others should not evoke the interaction anxiety and other negative emotions for the observer that actual participants in intergroup contact might” (p. 74). As Stephan and Stephan (1985) first underscored, the experience of anxiety during the interaction can have devastating outcomes for intergroup relations (e.g., Shelton, West, & Trail, 2010). Hence, since more indirect types of contact do not require the individual to be part of what may constitute for him or her an anxiety-provoking incident, this, according to Wright et al. (1997), speaks to the capacity of more indirect forms of contact as alternative means of reducing prejudice.

The aforementioned contentions of Wright et al. (1997) essentially pertain to how the two types of interaction (i.e., direct vs. indirect), are being experienced (see anxiety) or being perceived (see group salience) by the individuals in them. Their claim is that all else being equal, indirect forms of contact are less likely to cause anxiety and more likely to induce group salience than direct forms of contact, both of which are needed for intergroup

interactions to be fruitful. Despite these being clear hypotheses put forward by Wright et al. (1997) no study has yet tried to provide support for either of them. This is a task that I undertook in the studies presented in this chapter.

Lastly, in their article introducing imagined contact, Turner et al. (2007c) concluded by saying that: “Given that imagined contact is less direct than actual intergroup contact *and* extended contact, we might expect it to have a weaker and more temporary effect on intergroup bias than either of these interventions” (p. 438). More recently, however, imagined contact researchers argued that imagined contact is the only available type of indirect contact that involves the self directly and that in this sense it resembles direct contact (see Crisp & Husnu, 2011). There is also the argument that imagined contact should be able to bring about effects resembling the effects of direct contact (see Turner et al., 2013). Underlying these statements is a comparison not only between imagined and direct contact, but also between imagined and extended contact. In this case too, even though the comparison was hinted at or explicitly asked for (see Crisp & Husnu, 2011; Crisp & Turner, 2013), it has not been, to date, investigated. Having identified this as a gap in the literature, the comparison between the two subtypes of indirect contact, i.e. imagined contact and extended (more specifically vicarious) contact was the final research goal of this set of studies and was addressed in the last experiment presented in this chapter.

Current studies

To summarise, the main research goals addressed in the following experiments were: 1) assessing whether the prejudice-reduction effects of indirect contact are comparable to the effects of direct contact, 2) following-up the immediate effects procured by direct and indirect contact in order to determine and compare the longevity of these effects, but also to register

any effects that have a delayed onset, 3) addressing the hypotheses of Wright et al. (1997) that indirect forms of contact in comparison to direct contact yield less anxiety and higher group salience at the time of the interaction, and 4) comparing the magnitude, the breadth, and the longevity of the effects of imagined and vicarious contact.

In order to meet these goals, a pretest-intervention-posttest experimental design with random allocation of participants in the experimental conditions was used where the variables of interest were measured before the intervention and after it. To address the first research goal, I compared direct and indirect forms of contact with regards to their effectiveness in: a) improving outgroup attitudes (Experiments 1, 2, and 3), b) reducing intergroup anxiety (Experiments 2 and 3), and c) leading to more positive action tendencies towards the outgroup (Experiment 3). The second research goal was met by including in Experiments 2 and 3 a follow-up measure a week after the posttest (i.e., a delayed posttest) which allowed me to assess whether the effects of the intervention remained a week after contact had taken place, and whether any of the intervention effects had a delayed onset. To tackle the third research question I included, at posttest, measures tapping how participants perceived the interaction (group salience), how they felt towards the outgroup (attitudes and inclusion of other in self), and how anxious they felt immediately before (pre-contact anxiety) and during the interaction (state intergroup anxiety). The last research question pertaining to the comparison of the two indirect types of contact was addressed by including in the final experiment (Experiment 3), an imagined contact condition, along with the vicarious and the direct contact conditions. In this way, both indirect forms of contact were contrasted with the direct contact condition but were also compared to each other.

All studies were conducted in Cyprus. The participants of all experiments were Greek Cypriot students, and the outgroup (Turkish Cypriots) was kept constant across all studies. Whereas all experiments had the same core structure, each subsequent experiment formed a replication as well as an extension of the previous experiment. The experiments were thus nested, and they will be presented and discussed in relation to each other.

Experimental set-up

Even though the experimental manipulation of the various types of contact will be presented in detail later, I will briefly outline some key features of the experimental set-up, which were largely inspired by Studies 3 and 4 of Wright et al. (1997) and similar studies reported by Wright, Aron, and Brody (2008). In these studies, experimenters artificially created either an intergroup conflict situation (Wright et al., 1997, Study 3) or a situation of ingroup discrimination via the minimal group paradigm (Wright et al., 1997, Study 4). They then either induced intimacy between some of the members of the two groups in a positive intergroup interaction and then asked them to share with the rest of their ingroup members their positive intergroup experience (see Study 3), or they had participants observe via a one-way mirror the interaction between a member of their ingroup and an outgroup member (who were in reality trained confederates). In some other studies (mentioned in Wright et al., 2008), participants watched either an ingroup member whom they were close to (i.e., an ingroup friend) or an ingroup stranger interacting with an outgroup member who was a confederate.

In these ways Wright and his colleagues studied what they termed ‘extended contact’. One should notice, however, that while in Wright et al.’s (1997) Study 3, extended contact was essentially operationalised as ‘*learning* about an ingroup member who has an outgroup friend’ or rather ‘learning about an ingroup member’s positive experience with an outgroup member’, in Study 4 of

Wright et al. (1997) and in the studies reported by Wright et al. (2008), the experimenters ended up testing what nowadays is termed ‘vicarious contact’ (i.e., *observing* an ingrouper interacting with an outgroup(er)). What is more, in all of the aforementioned studies, the focus of Wright and his colleagues was on the participants assigned to the extended (or vicarious) contact condition. Thus they were not interested in those individuals who had direct contact with the outgroup(er), and consequently the impact the direct contact had on these individuals was not measured. Lastly, in these studies the groups were artificially created in the lab.

In the set of studies that follow, I utilized an experimental set-up similar to that described above but with real groups instead of artificially constructed groups. The basic experimental procedure utilized in my studies entailed the recruitment of dyads of friends who were randomly assigned to either the direct contact condition or the vicarious contact condition. Individuals in the direct contact condition had what was designed to be a positive and intimate interaction with an outgroup(er) (a confederate). This was done via the Closeness Induction Task of Sedikides, Campbell, Reeder, and Elliot (1998) (see also Study 3 of Wright et al., 1997). The participants in the vicarious contact condition on the other hand watched (via a one-way mirror or a monitoring device) and listened to (through an intercom system) their friend’s interaction with the outgroup(er). In this way I simultaneously manipulated these two types of contact in the same design. As far as I am aware, there are no other studies in the literature that performed a simultaneous experimental comparison of these two types of contact. The main challenges of this procedure are discussed next.

Methodological challenges

The first methodological challenge with statistical implications was related to using dyads of friends as participants in my studies. The use of dyads requires that the dyad

becomes the unit of analysis and not the individual, as the data of the members of each dyad are not typically independent of each other (see Kenny, Kashy, & Cook, 2006). I, however, deemed that the individual should serve as the unit of analysis for the following reasons: 1) while the participants registered for the studies as dyads, all questions tapped their individual experiences, and there were no measures in which participants were asked to answer questions about each other, 2) I checked for the likelihood of the non-independence of the observations before automatically treating these data as interdependent. Since I had distinguishable dyads, the designated non-independence test was the calculation of the relevant Pearson (r) correlations. None of the correlations for any of the experiments I present here was significant. The correlation coefficients were notably small, and their p -values non-significant. In fact p -values ranged from 0.18 to 0.90 thereby providing no support to the premise that the data between the individuals in the dyad were interrelated, which suggested independence of the data. There is, however, a reference by Kenny⁴¹ that the correlation test is not a sufficiently powerful test when the sample is under 35 dyads. My sample sizes in the second and third experiments were under this suggested cut-off point, but in personal communications I had with the same Professor Kenny, and with Professor Charles Judd, they expressed the opinion that the fact that none of all correlations derived was significant rendered the use of individuals and not dyads as the unit of analysis, an acceptable procedure.

The second methodological challenge concerned the fact that the vicarious contact participants were not exposed to the exact same intergroup experience. The intergroup interaction was designed to be a positive intergroup experience and the Closeness Induction Task was found in previous studies (e.g., Gaertner & Schopler, 1998) to be effective in doing what it was designed to do, i.e., build intimacy between two strangers and therefore give them

⁴¹ see personal webpage: <http://www.davidakenny.net/dyad.htm>.

a positive interpersonal experience. In this way I trusted that all vicarious contact participants were exposed to a positive and intimate encounter. Moreover, the outgroup member was always the same person (a confederate) who was trained to give the same answers to all participants. However, to be more confident that the participants in the vicarious contact condition were exposed to roughly the same intergroup interaction, I asked the participants in the vicarious contact condition to report after the interaction, the amount of intimate information they thought their friend disclosed to the outgroup member, and the amount of close information the outgroup member disclosed to their friend⁴². My hypothesis was that in the case that the vast majority of the vicarious contact participants reported similar amounts of information being disclosed by the ingroup member and the outgroup member, that would be an indication that the intergroup experiences the vicarious contact participants were exposed to were similar in that they were intimate interactions between an ingroup member and an outgroup member. I chose these measures (i.e., amount of intimate information disclosed by the ingroup member and amount of intimate information disclosed by the outgroup member) as opposed to measures tapping how positive they thought the interaction was for example, because I thought that reporting the amount of intimate information disclosed required from the participants an answer that was not as affected by their subjective experience of the interaction.

Analysing pretest-posttest designs

In the set of experiments I will present next, I utilized the same core experimental design, namely a randomized pretest-posttest design where participants in the various conditions were tested before the intervention and after it. The use of the pretest-posttest design with or without a control condition, according to Dimitrov and Rumrill (2001), has been widely used in behavioural research since it enables the comparison of groups and the

⁴² This measure also functioned as a test of the effectiveness of the Closeness Induction Task.

measurement of change as an effect of experimental treatments. In this section I will, in some detail, discuss the main ways that have been employed in the past to analyze pretest-posttest designs and the benefits and setbacks of each one of them. In this way I will be able to justify my decision for the method I used to analyse my data.

Three statistical methods have been predominantly employed in the literature to date for analysing pretest-posttest data. These are: 1) one-way Analyses of Variance where the dependent variable is the difference (gain) score (i.e., the difference between the pretest and the posttest scores) and Condition is the independent variable, 2) Repeated Measures Analysis of Variance, where Time of Measurement (pretest vs. posttest) is treated as a within-subject variable and Condition is treated as a between-subject variable, and 3) Analysis of Covariance, where pretest scores are added as a covariate, posttest scores become the dependent variable, and Condition is the independent variable. It should be noted that all these methods will only produce unbiased estimates if the following assumptions hold: 1) the dependent variable (in this case the posttest score or the change score) is normally distributed in all groups, 2) there is independence between the scores of each participant, 3) the population variance for each variable is equivalent for all groups (Rausch, Maxwell, & Kelly, 2003).

The first method (one-way Analysis of Variance on the difference scores, or change scores as they are often referred to) have been heavily criticized in the past for two main reasons: a) they are not as reliable as the raw scores (e.g., Cronbach & Furby, 1970) especially when the pretest and the posttest scores are highly correlated (Lord, 1967), b) they can be greatly biased when the phenomenon of the regression towards the mean occurs. Regression to the mean occurs when a first measurement on a variable is extreme and the score on a

subsequent measurement regresses to the mean. The effect this has on change scores is that the pretest-posttest differences are attenuated, something that is likely to result in biased estimates (Burr & Nesselroade, 1990). It should be noted that these two criticisms have been previously addressed (see, Allison, 1990), and change scores continue to be widely used for the analysis of change. Oakes and Feldman (2001) found that in the absence of randomization, or when there are baseline differences between the groups, change scores models are more likely to yield less biased estimates (if biased at all) in comparison to the rest of the techniques.

The second method (Repeated Measures Analysis of Variance) essentially treats the specific experimental design as a mixed design with Time of Measurement (pretest vs. posttest) being a within-subjects variable and Condition (e.g., experimental vs. control condition) being a between-subjects variable. According to Dwyer and Todman (1995) one of the problems with Repeated Measures Analysis of Variance is that it is based on the assumption that the within-factor levels (i.e., pretest and posttest) are randomized within subjects. This is, however, not a valid assumption as the pretest and the posttest have a specific order in time in a pretest-posttest design. They are not in other words randomized. Dimitrov and Rumrill (2001) describe this method as “not a healthy practice” (p. 162) because, as previous researches showed (e.g., Jennings, 1988), the Repeated Measures Analysis of Variance can be a confusing test with misleading results. The Repeated Measures Analysis of Variance checks for three possible effects, a main effect of Condition across all times of measurement, a main effect of Time of Measurement across all levels of the Condition variable, and an interaction between Condition by Time of Measurement. The two main effects essentially store no useful information for the researcher. For example a main

effect of Condition tells one whether the experimental group differs from the control condition on average across the pretest and the posttest. This forms a very conservative test for the intervention as the pretest is not meant to be affected by it, so one should expect no differences at pretest between the different levels of the Condition variable anyway. The interaction effect is the one that holds the most important information since it addresses the question of whether there are differences across the conditions in the change between pretest and posttest. This effect, however, is *identical* to the effect derived by the one-way Analysis of Variance on the difference scores (see, Huck & McClean, 1975) in which case one could simply perform difference scores to begin with.

The method that emerged as the most appropriate one to use for pretest-posttest designs is the last one, the Analysis of Covariance (Dimitrov & Rumrill, 2001; Duyard & Todman, 1995; Huck & McClean, 1975; Rausch, et al., 2003). According to Duyard and Todman (1995), the Analysis of Covariance gives the pretest its correct status by placing it as a covariate. In this way it answers the question of whether the levels of the Condition differ from one another at posttest after controlling for their pretest scores. According to Oakes and Feldman (2001), at least for a randomized design, the Analysis of Covariance will have superior power to the difference-scores models and it will yield unbiased estimates⁴³. One should be aware that there are three additional assumptions that should hold for the Analysis of Covariance, and substantial departures from these assumptions are likely to result in biased estimates. These additional assumptions are: 1) there is a linear relationship between pretest and posttest, 2) there is no measurement error in the dependent variable (i.e., posttest scores) or the covariate (i.e., pre-test scores), and 3) there is homogeneity of the regression slopes:

⁴³ For a detailed explanation of why Analysis of Covariance forms a more powerful test in comparison to the other two see Rausch et al. (2003).

i.e., the relationship between the covariate and the outcome is the same across all the levels of the independent variable. According to Rausch et al. (2003), however: “...when participants are randomly assigned to groups and the covariate is collected prior to the start of the treatment the estimate of the treatment effect is still unbiased when either the measurement error exists or one does not account for a non-linear relationship” (p. 469). Consequently, given random assignment, the only assumption that remains to be tested in order for one to feel confident that the results produced by the Analysis of Covariance are not biased, is the assumption of the homogeneity of the regression slopes.

I decided to employ the Analysis of Covariance method to analyze the following experiments since there seems to be a general agreement that it is the most appropriate method to utilize especially when a randomized design is in use. In the analyses presented next, Condition was the independent variable, pretest scores were the covariate, and posttest scores the dependent variable. This allowed me to determine whether the levels of the Condition variable differed in terms of their posttest scores. This question (i.e., “Do groups differ in their posttest scores?”) according to Rausch et al. (2003), is, within a randomized design, equivalent to the question of whether groups change differently from pretest to posttest.

To ensure success of random allocation to the various conditions, I compared the levels of the Condition variable to one another on their pretest scores for each dependent variable but also on a set of other variables that I deemed important⁴⁴. The assumption of the homogeneity of regression slopes was also tested via the use of the customized model in Analysis of Covariance. Following Field (2009), I tested the assumption of homogeneity of the regression slopes by specifying a model which included the interaction term between the

⁴⁴ This step is not entirely necessary as according to Rausch et al. (2003) the Analysis of Covariance is robust against “unhappy randomization” (Kenny, 1979, in Rausch et al., 2003). Unhappy randomization describes the phenomenon whereby groups differ at the levels of the pretest despite being randomized.

covariate (i.e., pretest scores) and the independent variable (i.e., Condition). A significant interaction term would indicate violation of the assumption of the homogeneity of the regression slopes, whereas a non-significant interaction term would mean that the assumption of the homogeneity of the regression slopes upheld and that the Analysis of Covariance estimates were free from bias.

Lastly, even though the main interest in these experiments was in exploring the differences across conditions, differences *within* conditions were also of interest. As Rausch et al. (2003) underline, differences *within* each condition can shed light on the differences occurring *between* conditions. In order to explore the changes over time *within* each condition, I used the Repeated Measures Analysis of Variance where Condition was treated as the between-subject variable and Time of Measurement (pretest vs. posttest) was treated as a within-subjects variable. I specifically asked for the pairwise comparisons between times of measurement (i.e., pretest to posttest) for each condition separately to see whether pretest to posttest changes occurred within each condition.

EXPERIMENT 1⁴⁵

In this experiment, the effectiveness of direct and vicarious contact in advancing more favourable outgroup attitudes was assessed and contrasted. This was achieved by assessing whether participants in each condition reported more favourable outgroup attitudes from pretest to posttest and whether the posttest outgroup attitudes differed in the two conditions while controlling for their pretest scores. Direct contact and vicarious contact were never compared in the past. Previous studies comparing direct and extended friendships,

⁴⁵ Experiment 1 was designed and run as part of my Master of Science thesis which I completed at the University of Oxford in year 2008. In order to incorporate this Experiment in my doctoral work I ran 20 more participants (10 pairs) and re-analysed the data.

nonetheless, have shown that direct friendships are more effective in reducing prejudice than extended friendships (see e.g., Paolini et al., 2004; 2007). Direct and extended friendships are both instances of good quality contact and as such they are different from what I operationalise in this study as direct contact (a one-off positive interaction with an outgroup member as opposed to having an outgroup friend) and vicarious contact (watching a friend having a positive interaction with an outgroup member as opposed to having an ingroup friend who has an outgroup friend). Despite these differences, my expectation was that direct contact (just like direct friendships) would have more profound effects on outgroup attitudes in comparison to extended (in this case vicarious) contact, but that both contact types would lead to more favourable attitudes at posttest in comparison to the pretest.

Furthermore, the two types of contact were compared in terms of whether they instigated different levels of group salience, different levels of state intergroup anxiety, and different attitudes towards the outgroup member. The first two variables corresponded to the claims raised by Wright et al. (1997), according to which indirect contact is more likely to lead to greater group salience and lower state intergroup anxiety. The third variable (attitudes towards outgroup member) was added out of interest. I expected that the attitudes towards the outgroup member would reflect the results of the attitudes towards the outgroup. Hence, I expected participants in the direct contact condition to report more favourable attitudes towards the outgroup member in comparison to the participants in the vicarious contact condition.

Hypotheses

Hypothesis 1 (differences in how each type of contact was perceived): differences were predicted to occur in the way the participants in the two conditions experienced the intergroup interaction. I expected the participants in the vicarious contact condition to report greater

group salience (Hypothesis 1a) but less intergroup anxiety (Hypothesis 1b) at the time of the interaction. I expected participants in the direct contact condition, on the other hand, to report more positive attitudes toward the outgroup (Turkish Cypriot confederate) in comparison to the participants in the vicarious contact condition (Hypothesis 1c).

Hypothesis 2 (differences in outgroup attitudes across time measurements within each condition): I expected the participants' attitudes towards Turkish Cypriots to be more positive at posttest in comparison to their pretest attitudes for both conditions.

Hypothesis 3 (differences across conditions on posttest outgroup attitudes): I expected the posttest outgroup attitudes of the participants in the direct contact condition to be significantly more favourable than the respective attitudes of the participants in the vicarious contact condition (while controlling for their pretest attitudes).

Method

Participants

Sixty-eight (34 dyads) Greek Cypriot students participated in this study. All of them were undergraduate students studying at universities in Nicosia, the capital of the Republic of Cyprus. All participants were female⁴⁶.

Procedure

The participants were recruited in dyads of friends through announcements in undergraduate classes. The students attending each class were invited to participate along with a friend of theirs in a study on human relations; the actual theme of the study was not disclosed to avoid sampling bias. The criteria for inclusion were that the participants had to be: a) Greek Cypriots (there were no Turkish Cypriots in the classes but there were, on the

⁴⁶ Unfortunately age was not recorded, but as all participants were students in undergraduate programs, the vast majority (if not all) of them were between 18 and 21 years of age.

other hand, students of a different nationality, e.g., Greeks), b) female. Their friends joining them in the study had to be Greek Cypriot female as well. Gender was kept constant in order to control for any effects. The participants were told that the study consisted of two stages in which both members of the dyad had to first complete an online questionnaire independently and then a week later to attend a 30-minute experiment on ‘interpersonal relations’. The experiment, they were told, would consist of a 10-minute task followed by the completion of a questionnaire. Instead of a payment, five pairs were to win 30 Euro each, at a raffle.

The students willing to take part put down their friend’s and their own name on a recruitment-list so that e-mails with the link to the online questionnaire could be sent to them for completion one week before the experiment. The main purpose of the online questionnaire was to take the scores of the participants on the various measures of interest at pretest in order to compare them with their scores on the same measures after the intervention. The participants were asked to complete this questionnaire independently. They were furthermore asked to create their own codes comprised of their month of birth and the last four digits of their mobile number to use as a ‘password’ for answering the questionnaire. They were asked to remember this code as it was going to be asked from them again. The code was used to match the participants’ responses between pretest and posttest when the study was completed. A week later participants were invited in to come together to the lab for the experiment. It was made clear to them that answering the first questionnaire was a prerequisite for taking part in the experiment, so all the participants had responded to the pretest before coming to the psychology lab for the experiment. The one-week interval was not achieved in several cases due to practical difficulties. In these cases I kept a minimum interval of four days between the pretest and the intervention.

Manipulation of direct and vicarious contact

When the two participants arrived at the lab they were welcomed by the experimenter and given initial information on the experimental procedure. The participants were told that the experiment would last for approximately ten minutes and that it consisted of two conditions. They were told that one participant had to be in one condition (say Condition 1) and the other one had to be in the other condition (say Condition 2). The participants were asked to decide between them which condition each one preferred. This process was followed for purposes of random allocation to the experimental conditions.

The experimenter then informed the participants that the participant who chose to be in Condition 1 would be led first to the booth where she would be introduced to the task she had to complete. During that time the second participant was asked to wait in the first room until the experimenter led her into a second room. Condition 1 was in fact the direct contact condition. The participant who chose to be in this condition was led to a room equipped with a table and two chairs at opposite sides of the table. The chair facing the door had already been occupied by another person who was in fact a Turkish Cypriot confederate. The confederate employed for the study was a female Turkish Cypriot called Ayşe (typical Turkish name) who was in the same age range as the Greek Cypriot participants, and was, like them, a university student. The confederate was instructed to behave as a naïve participant throughout the experiment.

Once the experimenter and the direct contact participant entered the room the experimenter asked the participant to take her seat. While the instructions to that point were given in Greek, the mother-tongue of the Greek Cypriot participants, once the experimenter and the participant of the direct contact condition entered the room where the confederate was,

the experimenter informed the participant that she would give the remaining instructions in English⁴⁷. She then introduced the two individuals to each other by name: the experimenter told the Greek Cypriot participant that Ayşe studied at a university in Kyrenia, a well-known town in the north part of the island, and she subsequently informed the confederate that the participant was a student at a university in Nicosia. They were then told that their task was to engage in a conversation during which they would take turns in asking and answering each other three lists of questions. The three lists of questions were taken from the Closeness Induction Task of Sedikides et al. (1998; 1999). The goal of this task is to induce closeness by asking participants to engage in a structured conversation which encourages mutual disclosure. This is done by having participants ask and answer questions that are superficial in nature and then gradually grow to be more intimate.

The participants were instructed by the experimenter to engage in a natural conversation by using three lists of questions that were taped in front of each one of them. They had to start with the first list and advance to the third list. They were given one minute to go through the first list which was comprised of simple questions like: “How old are you?” and “What do you study?”. The second and third lists were comprised of more intimate questions like: “What would you like to do after graduating from university?” and “If you could travel anywhere in the world, where would you go and why?” (second list), and “What is one of your biggest fears?” and “What is your happiest early childhood memory?” (third list)⁴⁸. For the second and the third lists they were given three and five minutes respectively to go through them. The participant and the confederate were advised not to rush through the list but take their time to answer as many questions as they wished. The experimenter indicated

⁴⁷ This surprised the participants at the beginning but when they realized that English was then used for the ‘other girl in the room’ to understand they were somewhat more at ease with the shift in the language used.

⁴⁸ The three lists can be found in the Appendix 1.

the end of each time interval by entering the room and asking the two individuals to advance to the next list of questions. The confederate was instructed to prepare the answers to the questions which she had to memorize in order to give the same answers to each participant.

After the task was explained to the participant and the confederate, the experimenter left the room saying that she would leave them some minutes to skim through the questions. In that time the experimenter led the participant of the vicarious contact condition to a second room. This room was connected to the first room via a one-way mirror which allowed the vicarious contact participant to both watch and listen to their friend's conversation with the Turkish Cypriot confederate in the other room. The participant was asked to carefully watch and listen to the conversation and she was provided with the three lists of questions so that she could more easily follow the conversation. Then the experimenter returned to the first room, asked the direct contact participant and the confederate whether they had any questions on the task before asking them to make a start with the first list of questions.

When the two individuals concluded the task, the experimenter returned to the room to signify the end of the conversation and to let them know that in order to complete the experiment they had to fill in one more questionnaire but that they had to do so in different rooms. The participant was then taken to a third room where she was given the questionnaire for completion whereas the confederate remained in the same room ostensibly filling in her own questionnaire. The experimenter then went to the second room where the 'vicarious contact' participant was, to inform her that she would also have to complete a final questionnaire in order for the experiment to be completed. She was taken to a fourth room to fill in the questionnaire so that she would not keep looking at the confederate who was still at

the first room. When the two participants were completing the final questionnaire the confederate got ready for the next pair of participants.

Measures

The pretest questionnaire contained the dependent measure (outgroup attitudes) along with some filler items in order to disguise the emphasis on intergroup relations and in order to protect the study as much as possible against demand characteristics. These filler items measured the participants' personality traits and also attitudes towards outgroups other than Turkish Cypriots. Finally, a measure of the participants' previous direct contact was also included. The posttest questionnaire contained the same main measures as the pretest questionnaire, and also measures that tapped into the experience/perception of the intergroup interaction. All measures are presented below in detail.

Pretest questionnaire

Attitudes towards Turkish Cypriots: participants rated their feelings towards Turkish Cypriots on a feeling thermometer (Converse & Presser, 1986) ranging from 1, *0 degrees* (very cold feelings) to 11, *100 degrees* (very warm feelings).

Attitudes towards other outgroups: were measured in the same ways as attitudes towards the main outgroup. The rest of the outgroups were: political refugees, asylum seekers, gays, lesbians, and economic immigrants.

Prior direct friendships: one item asked how many Turkish Cypriot friends the participant had on a scale ranging from 1, *none* to 5, *more than ten*.

Personality items (see Appendix 2 for the complete measure): a shorter version of the Five Factor Inventory of Costa and McCrae (1985) was used to tap three factors: agreeableness measured via nine items (e.g., *I see myself as someone who is generally trusting*), extraversion

measured with nine items (e.g., *I see myself as someone who is outgoing and sociable*), and openness to experience measured with eight items (e.g., *I see myself as someone who is curious about many different things*). The Likert scale ranged from 1, *strongly disagree* to 5, *strongly agree*.

Posttest questionnaire

The feeling thermometer measuring attitudes towards Turkish Cypriots as well as the other outgroups was the same as in the pretest questionnaire. The measures added to this questionnaire were the following:

Group Salience: a 3-item measure was utilized in which participants rated on a 5-point Likert scale the extent to which they experienced the following during the interaction: 1) felt they were a member of the Greek Cypriot community, 2) felt that the other person (Turkish Cypriot confederate) belonged to a different group/ community than their own? 3) felt that they were a Greek Cypriot interacting (or observing the interaction of a Greek Cypriot) with a Turkish Cypriot. The scale ranged from 1, *not at all* to 5, *extremely much*. The Cronbach's alpha for this measure was 0.70.

State intergroup anxiety: a short (6-item) version of Stephan and Stephan's (1985) original intergroup anxiety measure was used where participants were asked to rate on a 5-point scale (1, *not at all*; 5, *extremely*), to what extent they felt: '*threatened*', '*anxious*', '*comfortable*' (reverse-coded), '*awkward*', '*safe*' (reverse-coded), '*at ease*' (reverse-coded) while interacting (or observing the interaction of their friend) with the outgroup. The Cronbach's alpha for this measure was low ($\alpha = .50$). For this reason I deleted, as the reliability analysis suggested, the items '*safe*' and '*awkward*'. This yielded a more acceptable Cronbach's α of 0.60.

Therefore the composite scores reported for this measure were the average of the four remaining items.

Attitudes toward the outgroup member (Turkish Cypriot confederate): participants were asked to rate their attitudes towards the Turkish Cypriot confederate in the same way they rated their attitudes towards the outgroup as a whole, i.e., via the feeling thermometer.

Disclosure of intimate information: disclosure was measured through two items asking how much information of an intimate nature the Greek Cypriot participant disclosed to the outgroup individual, and how much information of an intimate nature the outgrouper disclosed to the direct contact participant. The 5-point scale ranged from 1, *none* to 5, *very much*.

Design

The experimental design was a pretest-posttest randomized design with two treatment conditions (i.e., direct contact condition and vicarious contact condition) and no control condition.

Results

The analyses were divided into three sections. First, I compared the scores of the participants in the two conditions on their pretest attitudes as well as on the number of prior outgroup friendships they reported in order to check the success of random allocation to the two conditions. Second, I derived the descriptive statistics for the measures that tapped into how participants experienced the interaction overall, before proceeding to compare the direct and vicarious contact conditions on group salience, state intergroup anxiety, and attitudes towards the outgroup (see Hypotheses 1a-c). Third, I looked for pretest-posttest changes on the dependent variable for each of the conditions to test Hypothesis 2, and I assessed whether

the two conditions differed from each other with regards to the magnitude of the pretest-posttest change they brought about (Hypothesis 3).

Random allocation

Participants reported very few prior direct friendships, $M = 1.28$, $SD = .64$ and their attitudes towards the outgroup were neither positive nor strongly negative ($M = 5.21$, $SD = 2.13$). A Multivariate Analysis of Variance (MANOVA) using Pillai's trace, with Condition (direct contact vs. vicarious contact) as the independent variable and pretest outgroup attitudes as well as number of prior friendships as the dependent variables yielded a non-significant multivariate effect ($V = 0.01$, $F(2, 65) = .26$, $p = .76$) thus attesting to the successful random allocation of the participants to the two conditions.

How participants perceived contact (Hypothesis 1)

The participants reported low to moderate group salience ($M = 2.20$, $SD = .92$), and very low intergroup anxiety ($M = 1.36$, $SD = .37$) at the time of the interaction. They also reported very positive attitudes towards the outgroup, i.e., Turkish Cypriot confederate ($M = 8.37$, $SD = 1.18$) and fairly high amounts of disclosure both on behalf of the Greek Cypriot participant ($M = 3.53$, $SD = .76$) and on behalf of the outgroup member ($M = 3.40$, $SD = .67$). Out of the 34 participants in the vicarious contact condition 28 reported that their friends had disclosed 'some' to 'quite a lot (of)' intimate information to the outgroup, and 29 out of the 34 participants reported that the outgroup reported 'some' to 'quite a lot (of)' intimate information to their friend. This gave some support to my expectation that the participants in the vicarious contact condition perceived their friend's intergroup experience to be as intimate as experienced by the participants in the direct contact condition themselves. In other words, there was evidence of matched experiences and perceptions of intergroup intimacy.

Table 3.1. presents the means and standard deviations for the two conditions on the three variables on which they were compared: group salience, state intergroup anxiety, and attitudes towards the outgroup member. A MANOVA with Condition as the independent variable and the three variables of interest as dependent variables, yielded no multivariate effect of Condition ($V = .04$, $F(3, 63) = .84$, $p = .48$, partial $\eta^2 = .04$), thus indicating that no differences occurred between the participants in the direct and the vicarious contact conditions with regards to how salient they perceived the group memberships to be during the interaction (group salience), how anxiety provoking the interaction was for them (state intergroup anxiety), and how much they liked the outgroup member (attitudes towards outgroup). These findings did not provide support to Hypotheses 1a-c.

Pretest-Posttest changes within each condition (Hypothesis 2)

The pretest and posttest scores of the two conditions on the dependent variable are presented in *Table 3.1*. The repeated measures ANOVA with Condition (direct vs. vicarious contact) as the between-subject variable and Time of Measurement (pretest vs. posttest) as the within-subjects variable yielded a significant main effect for Time of Measurement ($F(1, 66) = 38.75$, $p < .001$, partial $\eta^2 = .37$). The time pairwise comparisons showed, in line with Hypothesis 2, a significant pretest-posttest change for both direct ($p < .001$) and vicarious ($p < .01$) contact. Outgroup attitudes at posttest were more positive in comparison to pretest attitudes.

Table 3.1. Means and standard deviations of the two experimental conditions on the interaction-related variables

	Direct Contact	Vicarious contact
	<i>M (SD)</i>	<i>M (SD)</i>
Group salience	2.18 (0.88)	2.40 (1.04)
State intergroup anxiety	1.50 (0.39)	1.47 (0.38)
Attitudes towards outgroup member	8.52 (0.93)	8.24 (1.37)
Pretest outgroup attitudes	5.15 (2.16)	5.26 (2.12)
Posttest outgroup attitudes	6.91 (1.60)	6.12 (1.68)

Differences across conditions (Hypothesis 3)

An analysis of covariance (ANCOVA) with Condition as the independent variable, pretest outgroup attitudes as the covariate, and posttest attitudes as the dependent variable was conducted to determine whether the two conditions differed in their posttest scores on outgroup attitudes. The interaction term between Condition and Pretest scores was non-significant ($F(1, 64) = 1.21, p = .28$) and so the assumption of the homogeneity of regression slopes was met. The ANCOVA yielded a main effect of Condition $F(1, 65) = 7.09, p < .05$, partial $\eta^2 = .10$). As Hypothesis 3 predicted, the direct contact condition yielded more positive posttest outgroup attitudes in comparison to the vicarious contact condition ($p < 0.05$). In other words the posttest attitudes of direct contact were significantly more favourable than the posttest attitudes of the vicarious contact condition after controlling for their pretest attitudes.

Discussion

The effects of direct and vicarious contact were compared experimentally in a sample of Greek Cypriot students who reported prior to the experiment having very few if any outgroup friends. The intergroup contact manipulation aimed at exposing two groups of individuals to two different types of cross-group interaction, namely direct (face-to-face) and vicarious contact which was operationalised in this experiment as watching a friend interacting with an outgroup member. Using the Closeness Induction Task (Sedikides et al., 1998; 1999), I aimed at a positive interaction where the two parties (ingroup member and outgroup member) mutually disclosed information of a personal nature to them. This was meant to induce closeness between them. The vicarious contact participant on the other hand could observe a positive and intimate interaction between her friend (an ingroup member) and the confederate (an outgroup member).

The interaction-related measures showed that the Closeness Induction Task was successful as both the ingroup member and the outgroup member were reported both by the direct and the vicarious contact participants alike to have disclosed to each other a large amount of intimate information. Furthermore, the Turkish Cypriot confederate was very much liked by direct as well as vicarious contact participants, and also the interaction was experienced by the participants as an anxiety-free event. Even though I expected both direct and vicarious contact participants to perceive the interaction as positive, I also expected to detect some differences between the participants in the two conditions.

The interaction was only viewed to some extent by participants in both conditions as an event occurring between two individuals coming from two different groups (moderate group salience). Differences in group salience were expected but not found. This finding is in

disagreement with Wright et al.'s (1997) hypothesis that group salience will be higher in a vicarious contact as opposed to a direct contact instance. Perhaps the reason this did not happen is that the participants in the vicarious contact condition were observing a friend and not a stranger interacting with an outgroup member. In this case, the low levels of group salience reported by participants in the vicarious contact condition may have been caused by the fact that the participants in this condition did not make the shift to perceiving their friend as an ingroup member, and by extension the confederate as an outgroup member and retained the interpersonal identities instead.

The results of this study showed furthermore that at least in retrospect, the participants in the two conditions did not report any differences in the anxiety they experienced while participating in as opposed to observing the intergroup interaction. This was an intriguing finding that did not provide support to Wright et al.'s (1997) claim that extended contact will trigger less negative emotions (including anxiety) to the individuals observing an interaction in comparison to individuals participating in it. I propose, however, that when Wright et al. (1997) were making this claim they did not have in mind an intergroup interaction as positive as the one the participants in this experiment had, even though in their article they specifically make reference to the observation of a cross-group 'friendship' which is, by definition, a positive and high quality instance of intergroup contact.

I anticipated also that direct contact participants would report even more favourable attitudes towards the confederate. This hypothesis received no support either. I assume that this is due to the fact that the ratings for the outgroup member were in any case very positive and there was, as a result of that, very little room for any differences between the two conditions to emerge. On the other hand, as expected, both types of contact led to more favourable outgroup

attitudes at posttest. In line also with my hypotheses, and in agreement with findings reported for the comparison of direct and extended friendships (e.g., Paolini et al., 2007), direct contact led to an even greater pretest-posttest change in outgroup attitudes than vicarious contact. Participants in the direct contact condition reported even more positive outgroup attitudes at posttest than participants in the vicarious contact condition.

While the findings of this experiment in terms of the effectiveness of both types of contact in yielding more favourable outgroup attitudes are undoubtedly positive, one may wonder how enduring the effect of each type of contact on outgroup attitudes really is. Is it an effect that will fade away with time or is it an effect that is strong enough to last? Additionally, while pretest functioned as a baseline for attitude change to be measured, an actual control condition was missing from this study. Arguably, the pretest-posttest changes in the two contact conditions can only be thought to be valid if they are significantly different from the respective pretest-posttest differences of a control condition. These two points were addressed in Experiment 2.

EXPERIMENT 2

Experiment 2 served as a replication and an extension of Experiment 1. As such the same pretest-posttest design was kept to assess whether direct and vicarious contact would have positive immediate effects on outgroup attitudes and whether these effects would be larger for direct contact. The measures tapping the interaction were included, as in Experiment 1, at posttest to check: a) whether participants in Experiment 2 experienced the interaction as positively as the participants in Experiment 1 did and b) to assess here too whether there were any differences between the two conditions on the variables: group salience, state intergroup

anxiety, and attitudes towards outgroup member for which no differences were found in Experiment 1.

To address the question concerning the longevity of the intergroup contact effects I included an extra measurement a week after the intervention (delayed posttest) thus extending the original pretest-posttest design to a pretest-posttest-delayed posttest design. The inclusion of one more time of measurement (delayed posttest) raised the question of how to analyze the data. I was essentially interested in looking into two possible types of changes: a) the pretest to posttest changes to test whether Condition had immediate effects on the participants' attitudes, and b) the pretest to the delayed posttest changes to assess whether there were any effects a week after the intervention (either as retained effects from the posttest or as sleeper effects that emerged only at the delayed measurement point)⁴⁹. I decided to retain the same analysis technique as the one utilized in Experiment 1, i.e., the Analysis of Covariance. To address the two questions, I conducted, as suggested by Rausch et al. (2003), two separate Analyses of Covariance. Both analyses had Condition as the independent variable and pretest scores as the covariate. The first analysis had posttest scores as the dependent variable to answer the question of whether the conditions differed at posttest and the second one had the delayed posttest as the dependent variable to test if the conditions differed a week after the interaction while controlling for their initial scores.

As I was also interested in the changes happening within each condition I used the Repeated Measures Analysis of Variance with Condition as the between-subjects variable and

⁴⁹ There are of course more questions that can be answered using this design, e.g., "Do groups differ from posttest to delayed posttest?" or "Do groups differ in any way over time?", but I deemed the questions I pose here to be the most relevant to my research goals. For similar approaches to pretest-posttest-delayed posttest designs, see Ammar and Spada (2006), Clark, Ehlers, Hackmann, McManus, Fennell, Grey et al. (2006); DeKeyser and Sokalski (1996).

Time of Measurement (pretest vs. posttest vs. delayed posttest) as the within-subjects variable, and specifically considered for the pairwise comparisons between times of measurement (i.e., pretest to posttest, posttest to delayed posttest, and pretest to delayed posttest) for each condition separately. As stressed earlier, differences *within* each condition can shed light on the differences occurring *between* conditions.

Another important addition in this experiment was the inclusion of a control condition to serve as a point of comparison for the contact conditions. For this purpose, when an effect of Condition was found, then planned contrasts in combination with post-hoc tests indicated where the differences occurred. There were two types of comparisons that were of interest: a) whether each type of contact led to significantly different effects from the control condition at posttest and at delayed posttest, b) whether the two types of contact differed from each other. In order to address the first point, simple contrasts were used where direct contact was compared to the control condition and then vicarious contact was compared to the control condition. Post-hoc comparisons were then consulted to determine whether there were differences between the two contact conditions.

Furthermore, one more dependent variable was added in Experiment 2 which was not included in Experiment 1, namely intergroup anxiety. Intergroup anxiety has been a well-investigated variable in intergroup contact research with its origins tracing back to the work of Stephan and Stephan (1985). While the effects of direct contact on anxiety had been already demonstrated (e.g., Islam & Hewstone, 1993), Wright et al. (1997) underlined that one of the assets of vicarious contact is that the observation of a positive relationship between an ingrouper and an outgroup member can reduce negative expectations about future interactions. While support was found for this hypothesis (e.g., Gomez et al., 2011; Paolini et al., 2004; Turner et

al., 2008), an experimental comparison of the anxiety reduction effects of direct and vicarious contact was never conducted. Given what has been argued so far about indirect types of contact more generally, the effects of indirect forms of contact are thought to be more likely to match direct contact's effects in variables that are related to preparing the individual for future contact (e.g., intergroup anxiety) rather than variables that have to do with the evaluation of the outgroup (e.g., outgroup attitudes) (see Crisp et al., 2008).

Hypotheses

As the inclusion of the delayed posttest is a novelty in the experimental study of intergroup contact, predictions with regards to the results in the delayed posttest were hard to make. The immediate effects of contact as recorded at posttest were expected to fade away to a certain extent with the passage of time for both types of contact. The important question was whether they would be wiped out completely, or whether they would still be traceable a week after the interaction. Another unknown factor was how the control condition was going to behave. One would think that, given nothing intervened between the pretest and the posttest, and the posttest and the delayed posttest for the control condition, the ratings of the participants on the various items would be invariant. Given this, only changes in the two contact conditions were expected to occur (due to the intervention) and the task of this experiment was to see whether these changes were substantial enough to lead the two experimental conditions to yield posttest and delayed posttest scores that would be significantly different from those of the control condition.

Hypothesis 1 (differences in how each type of contact was perceived): Despite finding no differences between the direct and vicarious contact conditions in group salience, state intergroup anxiety, and attitudes toward the confederate in Experiment 1, I repeated these

comparisons in Experiment 2 too, to see if there would be any differences from the results of the first experiment.

Hypothesis 2 (differences in outgroup attitudes across time measurements within each condition): I expected a replication of the results of Experiment 1 that attitudes would become more favourable from pretest to posttest for the contact conditions. I also predicted that no positive changes would be observed for the control condition (Hypothesis 2a). I expected a leveling-off or a slight worsening of attitudes for the two experimental conditions between posttest and delayed posttest, but I did not expect this change to be significant. I expected, in other words, the effects of the intervention to fade away somewhat but not completely. No differences were expected for the control condition (Hypothesis 2b), c) I anticipated attitudes at the delayed posttest to be more favourable than the pretest attitudes for the experimental conditions but not for the control condition (Hypothesis 2c).

Hypothesis 3 (differences in intergroup anxiety across time measurements within each condition): like with outgroup attitudes, I expected intergroup anxiety levels to drop between pretest and posttest for the two experimental conditions but not for the control condition (Hypothesis 3a). I expected these effects to level-off at the delayed posttest for the two experimental conditions. In other words, I did not expect any posttest-delayed posttest changes to occur for either the experimental or the control conditions (Hypothesis 3b). Lastly, I anticipated the pretest-delayed posttest comparison to show less anxiety at delayed posttest for the two experimental conditions but not for the control condition (Hypothesis 3c).

Hypothesis 4 (differences across conditions on posttest and delayed posttest outgroup attitudes): I predicted an effect of Condition on posttest scores while controlling for the

pretest scores on attitudes. Simple contrasts would indicate more favourable outgroup attitudes in the two contact conditions in comparison to the control condition at posttest while controlling for pretest scores (Hypothesis 4a). I also expected a significant difference between direct and vicarious contact conditions with direct contact yielding even more positive attitudes at posttest (Hypothesis 4b). I predicted that there would be a Condition effect for the pretest-delayed posttest comparison where the delayed posttest attitudes in the two contact conditions would remain to be more positive than the control condition while controlling for their pretest attitudes (Hypothesis 4c). If there were any differences between the two types of contact at the delayed test these would show more favourable attitudes for direct as opposed to vicarious contact (Hypothesis 4d).

Hypothesis 5 (differences across conditions on posttest and delayed posttest intergroup anxiety): I anticipated an effect of Condition on the pretest-posttest changes where the participants of the two contact conditions would report significantly less anxiety at posttest in comparison to the control condition (Hypothesis 5a). I did not have any concrete prediction on whether direct contact would yield an even greater anxiety reduction in comparison to the vicarious contact. Given, however, that I expected indirect contact more generally to have its most profound effects on variables pertaining at preparing the individual for future contact, my tentative hypothesis was that the two conditions would not have different effects on this variable. Lastly, a significant effect for Condition was expected for the pretest-delayed posttest comparison whereby the two contact conditions would have lower anxiety levels at delayed posttest in comparison to the control condition but would be undifferentiated from each other (Hypothesis 5b).

Method

Participants

Seventy-nine individuals participated in Experiment 2, 54 of whom (27 dyads) were randomly allocated to the two experimental conditions and 25 individuals formed the control condition. All of the participants were female and they were students at universities in Nicosia.

Procedure

The procedure followed was identical to Experiment 1 with a few notable differences. At recruitment, the experimenter invited potential participants to participate in dyads (of friends) at a study on intergroup relations. She also added that in case there were individuals who wanted to participate and did not have a friend who was interested right then, they could still indicate willingness to participate as an adjacent study to the first one would be conducted simultaneously, and that in this second study the experimenter sought individuals and not pairs of participants. In this way the experimenter could recruit dyads for the experimental conditions and individuals for the control condition. Participants were informed that there was a monetary prize of 210 Euros to be distributed to five of the pairs (30 Euros per pair) taking part in the ‘first study’ and to four individuals (15 Euros each) taking part in the ‘second study’.

The manipulation of direct and vicarious contact did not differ from Experiment 1. The only difference in the whole procedure was that upon completion of the experiment and after the participants had filled in the posttest questionnaire, they were taken back to the initial room where the direct contact participant was informed that her friend had the opportunity to

observe her interaction with the Turkish Cypriot (confederate). Without giving any more information on the nature and the purposes of the study, the experimenter informed the participants that in order for this study to be completed they would have to answer one more questionnaire which was going to be sent to them a week later. They were also told that once they had submitted their responses to the online questionnaire they would be sent an e-mail in which the study and its goals would be explained in detail (debriefing) and that they would automatically be included in the raffle for the monetary prize.

Control condition

The participants in the control condition who were ostensibly participating in an adjacent study were sent an e-mail explaining the procedure of the study. They were asked to respond to three questionnaires sent to them via e-mail once per week for three weeks. They were encouraged to reply to each questionnaire on the day it was sent to them as the experimenter wished to maintain a week interval between responses. They were also told that once they had submitted their responses to the final questionnaire an e-mail would be sent to them explaining the study and its goals and that they would automatically be entered into the raffle for the monetary reward. As the experimenter could monitor whether the participant had submitted their responses or not each time, up to two reminder e-mails were sent to the participants who did not respond immediately to the questionnaires.

Measures

The measures were exactly the same as in Experiment 1. The only difference this time was that a second dependent variable was added, intergroup anxiety. Intergroup anxiety was measured with an adapted version of Stephan and Stephan's (1985) original intergroup anxiety

measure which asked participants to rate on a 5-point scale (1, *not at all* ; 5, *extremely*), to what extent they would feel: “threatened”, “anxious”, “comfortable” (reverse coded), “awkward”, “safe” (reverse coded), “at ease” (reverse coded), if they were the only Greek Cypriot in a group of Turkish Cypriots. The Cronbach’s alphas for this measure over the three time measurements were acceptable ($\alpha_{\text{pretest}} = .85$, $\alpha_{\text{posttest}} = .85$, $\alpha_{\text{delayed posttest}} = .82$).

The measures of the interaction (group salience, state intergroup anxiety, attitudes towards the outgroup, and level of disclosure of the ingroup and the outgroup) used in Experiment 1 were retained and were therefore included in the posttest questionnaire. The Cronbach’s alpha for the multi-item measures, group salience and state intergroup anxiety, was $\alpha = .76$ and $\alpha = .74$ respectively indicating acceptable reliability. For this reason all items were used to create composite scores for these two variables.

The delayed posttest questionnaire was essentially identical to the posttest questionnaire but without the measures evaluating the intergroup interaction. The control condition was given the same three questionnaires as the two experimental groups. The only difference between the control condition and the experimental conditions was that in the posttest questionnaire for the control condition there were no items tapping the intergroup interaction, as the participants in this condition did not have any contact with the outgroup.

Design

The design utilized was a pretest-posttest-delayed posttest randomized design with two experimental conditions (direct contact condition and vicarious contact condition) and a control condition.

Results

The same plan of analyses was adopted as with the analyses for the first experiment. I first checked for the random allocation of participants to the three conditions. The second step was to test for differences between the two experimental conditions in terms of how they perceived the intergroup interaction (Hypothesis 1). The third step was to check for pretest-posttest, posttest-delayed posttest, and pretest-delayed posttest changes within each condition and for each dependent variable (Hypotheses 2 and 3) before comparing the three conditions on their posttest and delayed posttest scores on the two dependent variables (Hypotheses 4 and 5).

Random allocation

As in Experiment 1, the participants reported very few to no prior friendships with Turkish Cypriots ($M = 1.37$, $SD = .77$). Their reported outgroup attitudes at pretest were more towards the negative end ($M = 4.85$, $SD = 2.26$) and their anxiety regarding intergroup interactions with Turkish Cypriots was rather high ($M = 3.09$, $SD = 0.89$). The means and standard deviations for all conditions for the variables on which they were compared to assess random allocation are given in Table 3.2. Cronbach's alphas are also reported in the same table for the multi-item measures. A MANOVA using Pillai's trace yielded a multivariate effect for Condition ($V = .16$, $F(6, 150) = 2.19$, $p < .05$, partial $\eta^2 = .08$). Based on the new p -value (p -value of 0.05 was corrected for the multiple comparisons; the new p -value was 0.02), one of the univariate effects was significant, the effect of Condition on the number of prior outgroup friendships ($F(2, 76) = 5.35$, $p = .007$, partial $\eta^2 = .12$). Post-hoc analyses with Bonferroni corrections indicated that the control condition participants had more prior

friendships in comparison to both the participants in the direct contact condition ($p < .05$) and the vicarious contact condition ($p < .05$).

Table 3.2. Means and standard deviations for all conditions on the variables used to test random allocation

	Direct Contact	Vicarious contact	Control
	<i>M (SD)</i>	<i>M (SD)</i>	<i>M (SD)</i>
Prior direct friendships	1.22 (0.64)	1.15 (0.46)	1.76 (1.01)
Pretest outgroup attitudes	4.48 (2.53)	4.93 (1.88)	5.16 (2.36)
Pretest intergroup anxiety	3.23 (0.88)	3.22 (0.90)	2.79 (0.83)

Since the conditions differed on the number of prior outgroup friends, this variable had to be controlled for in any analysis attempting to compare the three conditions. The variable ‘prior outgroup friendships’ therefore was added as a covariate to the two ANCOVAs (reported below) by which the posttest and delayed posttest effects of the three groups were compared. It should be noted here that the practice of controlling for third variables on which randomized conditions differ has been contested (see Miller & Chapman, 2001, for a review). However, according to Miller and Chapman (2001), a variable on which randomized conditions vary can be added as a covariate in an Analysis of Covariance testing the effect of the group on the dependent variable given one is confident that the differences on this variable arose by chance. In this case, Miller and Chapman (2001) argue, the Analysis of Covariance “will be removing noise variance from the group and not anything substantive about the group” (p. 45). Along the same lines, Overall and Woodward (1977) argued that the variable on which conditions differ can be added as a covariate when the grouping into the various

conditions is not the cause of the differences observed on this variable. I therefore felt confident in proceeding with controlling for prior direct friendships as I had no grounds to believe that the individuals in the control condition would differ from the participants in the experimental conditions on their prior friendships with the outgroup.

How participants perceived the contact manipulation

At posttest, the participants in the two contact conditions perceived the interaction to be, to some extent, an event between two salient social groups ($M = 2.31$, $SD = .85$). They reported on average rather low intergroup anxiety towards the confederate ($M = 1.94$, $SD = .72$) and positive attitudes towards the confederate ($M = 7.93$, $SD = 1.32$). The levels of disclosure were high both on behalf of the outgroup member ($M = 3.50$, $SD = .88$) and on behalf of the ingrouper ($M = 3.37$, $SD = 1.01$). Of the 27 participants in the vicarious contact condition 20 of them reported that their friends shared with the outgrouper ‘*some*’ to ‘*quite a lot (of)*’ intimate information and 22 out of the 27 participants in the same condition reported that the outgrouper shared with their friend ‘*some*’ to ‘*quite a lot (of)*’ intimate information. The fact that the majority of the participants in the vicarious contact condition reported the same amount of intimate information on behalf of their friends and the outgrouper reinforced my belief, as in Experiment 1, that the participants in the vicarious contact condition were exposed to very similar intergroup events.

Table 3.3 presents the means and standard deviations of the two experimental conditions on the variables of interest, i.e., group salience, state intergroup anxiety, and attitudes toward the outgroup member. The MANOVA using Pillai’s trace, with Condition as the independent variable and the aforementioned variables as the dependent variables, yielded

a non-significant multivariate effect ($V = .09$, $F(3, 50) = 1.55$, $p = .21$, partial $\eta^2 = .09$). None of the univariate effects was significant either.

Table 3.3. Means and standard deviations of the two experimental conditions on the interaction-related variables

	Direct Contact	Vicarious contact
	<i>M (SD)</i>	<i>M (SD)</i>
Group salience	2.23 (0.71)	2.43 (0.97)
State intergroup anxiety	2.03 (0.73)	1.89 (0.71)
Attitudes towards outgroup confederate	7.69 (1.23)	8.15 (1.41)

Pretest - posttest - delayed posttest changes (Hypotheses 2 and 3)

In order to address Hypotheses 2 and 3, I conducted two repeated measures ANOVAs, one for each dependent variable with Condition (direct contact vs. vicarious contact vs. control condition) as the between-subjects variable and Time of Measurements (pretest vs. posttest vs. delayed posttest) as the within-subjects variables and asked for the pairwise comparisons (pretest-posttest, posttest-delayed posttest, pretest-delayed posttest) within each condition. The means and standard deviations (SDs) of each condition on the two dependent variables are given in *Table 3.4*. The comparisons will be reported for each dependent variable separately.

Outgroup attitudes (Hypothesis 2)

For attitudes towards Turkish Cypriots, the repeated measures ANOVA yielded a significant effect of Time of Measurement ($F(2, 150) = 18.59$, $p < .001$, partial $\eta^2 = .20$.) Pairwise comparisons showed, in line with Hypothesis 2, significant pretest-posttest changes

for the two contact conditions with the posttest outgroup attitudes being more favourable in comparison to the participants' initial (pretest) attitudes (direct contact condition: $p < .001$; vicarious contact condition: $p < .01$). Unlike what was expected, the pretest-posttest comparison was significant for the control condition too ($p < .05$) with the difference, however, being that participants in the control condition reported *less* favourable attitudes at posttest. This finding was not in line with Hypothesis 2a which predicted no changes for the control condition.

While Hypothesis 2b predicted attitudes to remain unchanged or become slightly but not significantly more negative at the delayed posttest for the two contact conditions, the pairwise comparisons yielded significant posttest-delayed posttest changes for both contact conditions. Participants in the direct ($p < .001$) and the vicarious ($p < .001$) contact conditions reported *less* favourable attitudes at the delayed posttest. No significant posttest-delayed posttest changes occurred for the control condition ($p = .76$). Hypothesis 2c was also left unsupported as the pretest-delayed posttest comparison yielded no differences for the direct ($p = 1.00$) and the vicarious ($p = 1.00$) contact conditions but significant results for the control condition ($p < .001$). Participants in the control condition reported *less favourable* outgroup attitudes at the delayed posttest in comparison to their pretest attitudes.

Intergroup anxiety (Hypothesis 3)

With regards to intergroup anxiety, the repeated measures ANOVA yielded a significant main effect of Time of Measurement ($F(2, 150) = 6.99, p < .01, \text{partial } \eta^2 = .09$). The pretest-posttest pairwise comparisons within each condition yielded results that were largely in line with Hypothesis 3a: significant pretest-posttest changes were detected for vicarious contact ($p < .05$) but only marginal changes were found for direct contact ($p = .08$).

No changes in intergroup anxiety levels from pretest to posttest occurred for the control condition ($p = 1.00$). As predicted by Hypothesis 3b, no changes were detected between posttest and delayed posttest for any of the conditions (direct contact: $p = 1.00$; vicarious contact: $p = 1.00$; control condition: $p = 1.00$). Partly in line also with Hypothesis 3c were the results for the pretest-delayed posttest comparisons. Pairwise comparisons indicated significant pretest-delayed posttest changes for vicarious contact ($p < .01$) but only marginal effects for direct contact ($p = .08$) and no changes for the control condition ($p = 1.00$).

Table 3.4. Means and standard deviations on all measurements of the dependent variables across three conditions*

	Direct Contact	Vicarious contact	Control
	<i>M</i> (SD)	<i>M</i> (SD)	<i>M</i> (SD)
Pretest outgroup attitudes	4.42 (2.56)	4.93 (1.88)	5.16 (2.36)
Posttest outgroup attitudes	6.04 (1.51)	5.96 (2.03)	4.08 (2.23)
Delayed posttest outgroup attitudes	4.54 (2.35)	4.70 (2.45)	3.72 (2.19)
Pretest intergroup anxiety	3.24 (0.90)	3.22 (0.90)	2.79 (0.83)
Posttest intergroup anxiety	2.93 (0.97)	2.82 (0.77)	2.80 (0.88)
Delayed posttest intergroup anxiety	2.99 (0.90)	2.76 (0.69)	2.83 (0.91)

* Note that these means have not been adjusted for the covariate prior direct friendships

Differences across conditions

In order to compare the three conditions on the changes they produced across time-measurements, I conducted two ANCOVAs, one for each dependent variable. Both of these analyses had Condition as the independent variable and pretest scores of the corresponding dependent variable along with prior direct friendships as the covariates. The first analysis had

posttest scores as the dependent variable to assess whether there were differences across conditions on posttest attitudes and intergroup anxiety while accounting for their pretest attitudes and intergroup anxiety respectively. The second analysis had delayed posttest scores as the dependent variable and assessed whether the three conditions differed with regards to their delayed posttest attitudes and intergroup anxiety while controlling for pretest levels on these variables. The results of the two analyses are reported first for outgroup attitudes and then for intergroup anxiety.

Outgroup attitudes (Hypothesis 4)

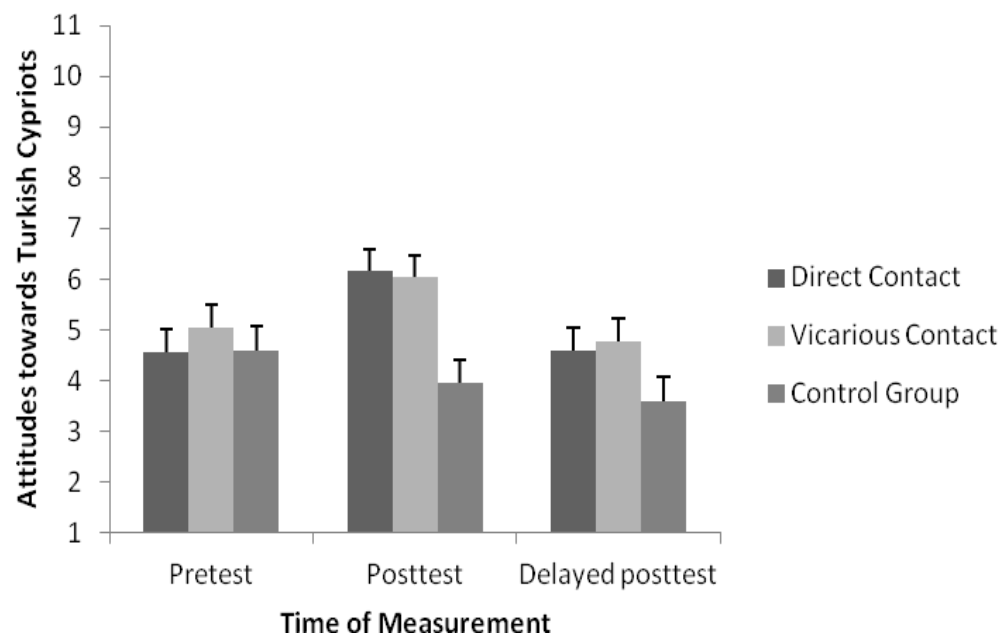
Figure 3.1. shows the pretest, posttest, delayed posttest scores on outgroup attitudes for each condition using the estimated marginal means, i.e., the means after being adjusted for the covariate (prior outgroup friendships). For the first ANCOVA the interaction term between Condition and Pretest attitude scores on posttest attitudes was significant ($F(2, 70) = 4.39, p < .05$) which meant that the assumption of the homogeneity of regression slopes was violated. The results of this analysis should therefore be interpreted with caution as the estimates provided might be biased. The ANCOVA yielded an effect for Condition ($F(2, 74) = 21.84, p < .001, \text{partial } \eta^2 = .37$). Simple contrasts⁵⁰ indicated in line with Hypothesis 4a, a significant difference between direct contact and the control condition ($p < .001$) and vicarious contact and the control condition ($p < .001$) at posttest. Contrary to Hypothesis 4b, post-hoc analyses revealed no differences between direct and vicarious contact ($p = .73$). The finding of Experiment 1 therefore, showing direct contact yielding more positive attitudes at posttest in comparison to vicarious contact, was not replicated here.

The second analysis using delayed posttest attitudes as the dependent variable showed a non-significant interaction between Condition and Pretest scores ($F(2, 69) = .82, p = .45$)

⁵⁰ As simple contrasts are non-orthogonal a more conservative p -value was adopted ($p < .02$).

which meant that the assumption of homogeneity of the regression slopes held for this analysis. It moreover yielded a significant effect for Condition ($F(2, 73) = 5.41, p < .01$, partial $\eta^2 = .13$). Simple contrasts indicated significant differences between direct contact and control condition ($p < .01$) and between vicarious contact and control condition ($p = .01$) with the two contact conditions yielding more favourable attitudes. These results were in line with Hypothesis 4c. Post-hoc analyses yielded no differences between the two contact conditions at delayed posttest either ($p = 1.00$) (see Hypothesis 4d).

Figure 3.1. Pretest – posttest – delayed posttest scores of each condition on outgroup attitudes*



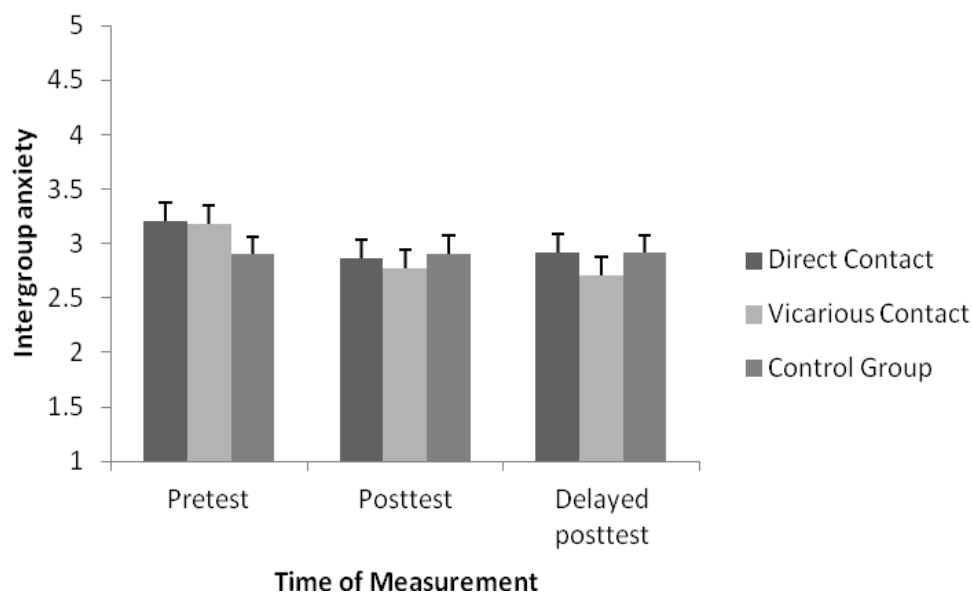
* The means presented here are adjusted for the covariate prior direct outgroup friendships

Intergroup anxiety (Hypothesis 5)

Figure 3.2. presents the pretest, posttest, and delayed posttest means of each condition on intergroup anxiety. These are the estimated marginal means which were derived after

controlling for prior direct friendships. For the first analysis of covariance, the interaction between Condition and Pretest scores on posttest intergroup anxiety was non-significant ($F(2, 70) = 2.66, p = .08$) thus showing that the homogeneity of regression slopes assumption was confirmed. Unlike what was predicted by Hypothesis 5a, the Analysis of Covariance did not yield an effect for Condition ($F(2, 74) = 1.64, p = .20$, partial $\eta^2 = .04$), so the anxiety levels reported at posttest by the participants of the two contact conditions were not significantly different from the corresponding means of the control condition while controlling for the pretest intergroup anxiety levels. These findings did not provide support to Hypothesis 5a.

Figure 3.2. Pretest – posttest – delayed posttest scores of each condition on intergroup anxiety*



* The means presented here are adjusted for the covariate prior direct outgroup friendships

The interaction between Condition and Pretest scores on intergroup anxiety at the delayed posttest was marginally significant for the second ANCOVA ($F(2, 69) = 2.98, p =$

.06). This result offered only weak support that the assumption of homogeneity of regression slopes held. The analysis yielded a marginal effect for Condition ($F(2, 73) = 2.80, p = .07$, partial $\eta^2 = .07$). A look at the simple contrasts indicated significant differences between the vicarious contact condition and the control condition ($p = .02$) with less anxiety at the delayed posttest being reported by the vicarious contact participants in comparison to the control condition participants. This finding was only partially in line with Hypothesis 5b which predicted a significant difference between both contact conditions and the control condition at the delayed posttest. Contrary to what was anticipated, direct contact did not yield lower anxiety at delayed posttest in comparison to the control condition even though there was a trend in that direction ($p = .19$). The effects of the two contact conditions were not different from each other ($p = .80$).

Discussion

Experiment 2 aimed to examine whether intergroup contact (direct or vicarious): a) led to immediate as well as delayed changes in two variables of interest, i.e., outgroup attitudes and intergroup anxiety, b) yielded significantly more positive attitudes and significantly lower anxiety at posttest and delayed posttest in comparison to the control condition, and c) differed in terms of how the participants in each one of them perceived the interaction but also in terms of the magnitude of their immediate and delayed effects on the two dependent variables. The inclusion of a control condition and baseline (pretest) measures, as well as the inclusion of a delayed posttest, rendered this experiment a very strict test of the effectiveness of intergroup contact in reducing prejudice.

Just as in Experiment 1, high levels of disclosure were reported for the interaction, and the ratings of the outgroup member were favourable. Group salience was moderate, and state

intergroup anxiety was rather low, even though not as low as in Experiment 1. None of the expected differences occurred between direct and vicarious contact conditions (see Hypothesis 1). Group salience and state intergroup anxiety levels were undifferentiated, and the outgroup was equally liked. Even though the unequivocal findings coming from the two experiments are hard to contest, while conducting this experiment I paid more attention to noticing the reaction of the participants once it was announced that their task entailed having/observing a conversation with a Turkish Cypriot. The reactions of the participants were not stress-free. I therefore thought that the a posteriori measure of state intergroup anxiety I had employed so far probably might have not been serving its purpose. I suspected that this measure merely captured the memory the participants chose to store of their feelings throughout the interaction instead of the actual feelings they experienced. To me there seemed to be a good likelihood that participants made the inference (while completing this measure) that since the interaction was positive then they themselves must have been in a positive state too. They might have in other words suppressed or simply not reported any feelings they had that did not match the valence of the experience they had gone through.

In replication of the first experiment's findings, participants in both direct and vicarious contact conditions reported significantly more positive attitudes at posttest in comparison to pretest (Hypothesis 2a). Most importantly, their outgroup attitudes at posttest were significantly more favourable than the attitudes of the control condition (Hypothesis 4a). Unlike what was anticipated by Hypothesis 4b, and unlike the findings of Experiment 1, the results of this experiment showed no differences between the two contact conditions. Direct contact did not lead to significantly better attitudes at posttest in comparison to vicarious contact. Also contrary to my predictions (Hypothesis 2b) delayed posttest attitudes were

significantly worse than the posttest attitudes for the two contact conditions, thus showing that the effects of contact on attitudes faded away a week after contact. In fact outgroup attitudes reverted back to their pre-contact levels a week after contact and this is why there were no differences between pretest and delayed posttest attitudes for the two contact conditions. Interestingly, the delayed posttest attitudes of the two experimental conditions were significantly better than the attitudes of the control condition participants (see Hypothesis 3b). This effect, however, should be interpreted with caution as the attitudes reported by the control condition participants had an unexpected course. They got worse at each subsequent measurement. This led to their attitudes becoming more negative than the regressed-to-pretest-levels-attitudes reported by the participants in the contact conditions.

Notably, the pattern observed for outgroup attitudes for the control condition was not observed for the second dependent variable, intergroup anxiety. The levels of anxiety reported by the control condition participants remained unchanged over time, as was expected. Largely in line with my predictions (see Hypothesis 3a), intergroup anxiety levels reduced at posttest in comparison to pretest for the contact conditions (albeit only marginally for direct contact) and they remained reduced one week later (Hypothesis 3b). Contrary to Hypothesis 5a though the posttest intergroup anxiety levels of the two contact conditions were not significantly different from those reported by the control condition participants. Interestingly, however, and partly in line with Hypothesis 5b, vicarious contact but not direct contact led to significantly lower intergroup anxiety a week after interaction in comparison to the control condition. There were no differences between the two contact conditions at posttest or delayed posttest which was in line with my expectations.

The results of this experiment suggested that a one-off interaction with one outgroup member may have temporary effects on outgroup attitudes that are significant when compared to a no-contact control condition, but that these attitudes are not strong enough to hold over time. On the other hand, a single intergroup interaction was enough to reduce anxiety levels for the two contact conditions (albeit marginally so for direct contact). An important finding is that the anxiety levels did not revert back to pretest levels for the two contact conditions, and for vicarious contact they even became lower than the anxiety levels reported by the control condition one week after contact.

It should be noted here that the pretest anxiety levels reported by the participants in this study were markedly high. Participants also reported remarkably (albeit expected) low levels of good quality intergroup contact (i.e., outgroup friendships) which probably explains the high anxiety levels. These mean that at least amongst the segment of the population under study (Greek Cypriot youth) who report being highly anxious about engaging in future outgroup contact, intergroup anxiety provides fertile ground for contact interventions. Moreover, the success of vicarious contact in reducing intergroup anxiety in comparison to the control condition at the delayed posttest spoke to the usefulness of indirect types of contact in yielding positive outgroup outcomes *especially* with regards to preparing individuals for future contact via reducing anxiety. In line with these findings, I became very interested to, on the one hand, replicate these findings on anxiety reduction and also to see whether another subtype of indirect contact, imagined contact, would have effects comparable to the effects of vicarious contact on this variable. This was one of the aims of Experiment 3 which is presented next.

EXPERIMENT 3

Experiment 3 served as a replication and an extension of the previous two experiments, but primarily Experiment 2. While keeping essentially the same design, this experiment extended Experiment 2 in some important ways. First, one more type of indirect contact, imagined contact, was included in this experiment. Imagined contact has not yet been contrasted with other types of contact even though comparisons between direct and imagined contact as well as between imagined and vicarious contact have been alluded to or explicitly asked for (see Crisp & Turner, 2013; Turner et al., 2013). Imagined contact, like vicarious contact, was proposed to be a stepping stone to actual contact. Indeed, imagined contact was found to be able to reduce anxiety for intergroup interactions (e.g. Turner et al., 2007c; Turner et al., 2013; West et al., 2011), and to increase the intention of people to engage in cross-group interactions (e.g., Husnu & Crisp, 2010a; Turner et al., 2013, Turner & West, 2012). On the other hand, imagined contact was thought to entail similar conscious processes to the ones employed in direct contact and claims were made that this should result in imagined contact having similar intergroup outcomes to those brought about by direct contact (see Turner et al., 2013). Experiment 3 provided the setting to compare the effects of imagined contact with both direct and vicarious contact. The strong emphasis on intergroup anxiety and behavioural intentions as plausible effects of imagined contact caused me to, on the one hand, keep the measure of intergroup anxiety which I used in Experiment 2 and to add one more target variable, namely positive action tendencies.

Furthermore, consistent with one of the aims of the other two experiments, I strived to investigate in this experiment too whether the various types of contact were experienced in different ways by the participants in each condition. For this purpose I compared the three

types of contact on four variables: attitudes towards the outgroup (confederate/ imagined individual), group salience, inclusion of other in self, and pre-contact anxiety. The first two variables were included in the previous two experiments to assess whether direct and vicarious contact differed on them. No differences between the two variables were detected. I decided, however, to retain them in this experiment to see how imagined contact, in comparison to the other two conditions, affected them. An interesting difference between direct and vicarious contact, on one hand, and imagined contact, on the other, is that the outgroup is physically present in the first two types of contact. I assumed that it is easier to like an individual who is present than an imaginary individual all else being equal (i.e., the interaction one has with that individual is positive) – this assumption was thus tested via the inclusion of the feeling thermometer for the confederate/imagined individual. As for group salience, Wright et al. (1997), as mentioned before, proposed that group memberships would be more salient to an observer (of an intergroup interaction). This claim could stand in my opinion for imagined contact as well, since for this type of contact too, the individuals are being the observers of their own (as opposed to someone else's) interaction with the outgroup. I also think that because the imagined individual is not so individualized, group salience will be even higher for imagined contact.

The other two variables, inclusion of other in self and pre-contact anxiety, comprised a novelty of this study. Inclusion of other in self (see Aron & Aron, 1997; Aron et al., 1992) was first introduced to the intergroup contact literature by Wright et al. (1997) who expressed the opinion that since an ingroup is by default part of one's self, observing an intimate interaction of a close ingroup with an outgroup leads to the outgroup becoming, by extension, part of the observer's self. Even though the work of Wright et al. (1997) makes no

special reference to the ingrouper who engages in the interaction (direct contact) and whether/how much they include the other in themselves, the reasonable assumption to make based on prior research findings (e.g., Sedikides, Reis, & Olsen, 1993) is that for the individuals engaging in the interaction, the inclusion of other in the self will be even greater. As the ‘other’ (i.e., the outgroup) will be more likely to be included in oneself when the self is the actor in an intergroup interaction, this should be the case regardless of whether this interaction happens in real life or in the actor’s imagination. On the contrary when the self is not directly part of the interaction then including the outgroup in the self will not be as easy.

As for pre-contact anxiety, this new measure was added because in both of the previous experiments the anxiety experienced throughout the interaction was reported by both direct as well as vicarious contact participants to be conspicuously low. Even though the low anxiety reported might be reflecting well how the participants ended up perceiving the interaction, it is not very informative of how participants felt upon being told that their task involved entering (or observing) an intergroup interaction. One could argue here that if any anxiety had been experienced by the participants (and especially the direct contact participants), this must have probably taken place at the very early stages of the experiment when participants were *about* to engage in the intergroup interaction. In order to capture this, I included in the experimental process one more step whereby anxiety was measured once participants were told the task they had to complete. I expected this measure to better capture the higher anxiety the direct contact participants experienced more than the vicarious and imagined contact participants just before the interaction. I, nevertheless, kept the state intergroup anxiety measure which I used in the first two experiments in order to see whether

there would be any discrepancies between the anxiety the participants reported having before the task started and the anxiety they reported having throughout the task.

Three further amendments were made to this study in comparison to Experiment 2. The first amendment had to do with the treatment of the control condition. The control condition behaved somewhat unexpectedly in Experiment 2, at least with regards to the first dependent variable, outgroup attitudes. Participants in the control condition reported *less* favourable outgroup attitudes at every subsequent measure. Even though I could not tell whether this had to do with the way the control condition was treated, in Experiment 3 I attempted what I believed to be an improvement to the treatment of the control condition. This change was essentially inspired by control conditions that have been tried out in imagined contact experiments, where participants in the control condition are asked to merely think of an interaction with the outgroup. Hence, this time around I asked the control participants to *think* about intergroup contact by asking them whether they happened to have an interaction with the target outgroup (Turkish Cypriots) in the week between pretest and posttest, before proceeding to complete the posttest questionnaire.

The second amendment had to do with the measurement of prior contact. For this study I added one more measure of prior intergroup contact, asking participants how much contact they had in general with Turkish Cypriots without qualifying contact to be cross-group friendships. I noticed in the previous two experiments that the number of friendships reported by the participants was scarce to nonexistent. I therefore added this variable of ‘mere contact’ to assess whether the Greek Cypriot participants were only lacking direct friendships or whether they simply had not contact with Turkish Cypriots. I deemed that having this information would help me with the interpretation of my results.

Also, a delayed measure of the participants' attitudes towards the outgroup member was added to this experiment. As found in the previous two experiments, participants reported at posttest having very favourable attitudes towards the confederate. Given the results of outgroup attitudes in Experiment 2, however, where outgroup attitudes became remarkably more positive at posttest before reverting back to their pretest levels at delayed posttest, an interesting question to address is whether attitudes towards the individual outgroup member would suffer the same fate or not. A delayed measurement of the participants' attitudes towards the outgroup member allowed me to test this question.

Hypotheses

Hypothesis 1 (differences in how each type of contact was perceived): the three contact conditions were compared on four measures, attitudes towards the outgroup member, group salience, inclusion of other in self, and pre-contact anxiety. I predicted that direct contact would yield the most favourable attitudes towards the outgroup member, followed by vicarious contact, followed by imagined contact. In other words I expected participants to like more an outgroup member who was existent in real life than an imaginary outgroup member (imagined contact condition) but I still expected that direct contact participants would report the most positive attitudes (Hypothesis 1a). With regards to group salience, I anticipated imagined contact to yield the highest group salience, vicarious contact the second highest group salience, and direct contact to yield the least salience (Hypothesis 1b). Furthermore, I expected direct contact participants to report the highest inclusion of other in self followed by imagined contact participants, then vicarious contact participants (Hypothesis 1c). As far as pre-contact anxiety was concerned, I anticipated direct contact to elicit the highest pre-contact anxiety but expected no marked differences between vicarious and imagined contact in that respect

(Hypothesis 1d). Lastly I expected that there would be differences between the pre-contact anxiety and the state intergroup anxiety the direct contact participants reported but I did not expect this discrepancy to occur in the vicarious and imagined contact conditions (Hypothesis 1e).

Hypothesis 2 (differences in outgroup attitudes across time measurements within each condition): I expected a replication of the results of Experiment 1 and 2 that attitudes would become more favourable from pretest to posttest for the direct and vicarious contact conditions and that no positive changes would occur for the control condition. I additionally predicted that just like the other two contact conditions, imagined contact too would lead to more favourable attitudes towards the outgroup (Hypothesis 2a). In Experiment 2 the attitudes at delayed posttest reverted back to the pretest attitudes for the two contact conditions. I suspected that this would be the case for this experiment too. In other words I expected significant changes in attitudes between posttest and delayed posttest with the delayed posttest attitudes being worse than posttest attitudes (at least) for the direct and vicarious contact conditions whereas no changes were expected to emerge for the control condition (Hypothesis 2b). I anticipated attitudes at the delayed posttest to be undifferentiated from pretest attitudes (at least) for the direct and vicarious contact conditions as well as for the control condition (Hypothesis 2c). The delayed effects of imagined contact were exploratory and even though no strong predictions could be made about them, I suspected that it would not differ from the other two types of contact and that its effects on attitudes would weaken as time passed by (Hypothesis 2d).

Hypothesis 3 (differences in intergroup anxiety across time measurements within each condition): Based on the results of Experiment 2, I anticipated a reduction of intergroup

anxiety at posttest (in comparison to pretest intergroup anxiety) for all contact conditions (including imagined contact) but no changes for the control condition (Hypothesis 3a). I expected the immediate effects on anxiety to last in time for the three contact conditions and so I expected no changes between posttest to delayed posttest for them. No changes were expected for the control condition either (Hypothesis 3b). I anticipated the pretest-delayed posttest comparison to indicate less anxiety at delayed posttest for all contact conditions but not for the control condition (Hypothesis 3c).

Hypothesis 4 (differences in positive action tendencies across time measurements within each condition: Given the contention that imagined contact's greatest contribution lies in its effectiveness to increase willingness for future contact and since the findings so far are consistent with these contentions, I expected to find more positive action tendencies at posttest (in comparison to pretest) at least for direct and imagined contact and perhaps for vicarious contact too and expected no differences in the control condition (Hypothesis 4a). I expected a slight regression to pretest scores at the delayed measure which, however, would not result in action tendencies being worse in the delayed posttest in comparison to the posttest (Hypothesis 4b). Consequently, I expected the delayed action tendencies to be more positive than action tendencies at pretest particularly for the direct and imagined contact conditions (Hypothesis 4c).

Hypothesis 5 (differences across conditions on posttest and delayed posttest outgroup attitudes): I predicted an effect of Condition on posttest scores while controlling for the pretest scores on attitudes whereby the posttest attitudes of all contact conditions would be more positive than the attitudes reported by the control condition participants (Hypothesis 5a). I also expected direct contact to yield the most positive attitudes, followed by vicarious

contact, then imagined contact, and then the control condition (Hypothesis 5b). I anticipated no Condition effect for the pretest-delayed posttest comparison as I expected the effects on attitudes not to last in time for the contact conditions (Hypothesis 5c).

Hypothesis 6 (differences across conditions on posttest and delayed posttest intergroup anxiety): I anticipated a Condition effect on the pretest-posttest changes where the participants of all contact conditions would report significantly less anxiety at posttest in comparison to the control condition (Hypothesis 6a). I also predicted finding a significant effect for Condition for the pretest-delayed posttest comparison according to which once again all contact conditions would have lower anxiety levels at delayed posttest in comparison to the control condition (Hypothesis 6b). No differences across the different contact types were expected to occur on this variable (Hypothesis 6c).

Hypothesis 7 (differences across conditions on posttest and delayed posttest action tendencies): I expected a Condition effect on the pretest-posttest changes showing the participants of all contact conditions reporting significantly more positive action tendencies than the control condition (Hypothesis 7a). I furthermore anticipated that direct contact would yield the most positive action tendencies, with imagined contact coming second, vicarious contact third, and the control condition last (Hypothesis 7b). As for the pretest-delayed posttest changes, I predicted an effect for Condition for the pretest-delayed posttest comparison. I expected the participants in the direct contact condition and perhaps the imagined contact conditions to significantly differ from the control condition but I was not sure whether the effects of vicarious contact would be strong enough a week after interaction on this variable to be significantly different from the control condition (Hypothesis 7c).

Hypothesis 8 (posttest-delayed posttest attitudes towards the confederate and differences across conditions): Even though outgroup attitudes were expected to get worse from posttest to delayed posttest, this was not expected for the participants' attitudes towards the confederate/ imagined individual. I therefore expected no posttest-delayed posttest changes on this measure within each contact condition (Hypothesis 8a). If any differences occurred across conditions I would expect them to show more positive attitudes towards the outgroup for direct and vicarious contact in comparison to imagined contact (Hypothesis 8b).

Method

Participants

The sample consisted of 75 individuals, 19 dyads (38 individuals) comprised the direct and vicarious contact conditions, 20 individuals were in the imagined contact condition, and 17 individuals at the control condition. All of the participants were female and they were students at universities in Nicosia.

Procedure

As in Experiment 2, the experimenter invited students to participate in a study on interpersonal relations in dyads (of friends). The experimenter informed participants that the study had three phases, that for the first and the third phase they would have to complete a questionnaire online and that for the second phase they had to come along with their friend to the psychology lab for a 30-minute experiment. She also added that in case there were individuals who wanted to participate and did not have a friend who was interested right then, they could still indicate willingness to participate since an adjacent study to the first one was being conducted simultaneously. They were informed that this adjacent study was identical to

the first one but instead of using dyads of friends it sought individuals. The ‘second study’, the experimenter told the potential participants, consisted of three phases too, where the first and the third phases required the participants to complete an online questionnaire. They were also informed that for the purposes of the second phase of the study, half of the participants would have to come to the lab for a 30-minute experiment (imagined contact condition) whereas the other half (control condition) would not have to come to the lab but that they would be assigned a task online. The experimenter also announced that there was a monetary price of 270 Euros to be distributed to five pairs (30 Euros per pair) taking part in the ‘first study’ and to eight individuals (15 Euros each) taking part in the ‘second study’. They were informed that only the pairs/ individuals who completed all three phases would be included in the raffle.

As in the previous experiment, a one-week lag between measurements was attempted and in most cases was achieved. Participants were sent the pretest questionnaires one week before the experiment/ electronic task. Immediately after the experiment participants completed the posttest questionnaire, and one week later the delayed posttest questionnaire was sent to them electronically. Participants were not given any information on the study throughout its duration but were told that they would be debriefed upon completion of the last phase of the study. In those cases in which participants failed to complete the questionnaire on the day it was sent to them, they were sent a reminder the day after.

Contact manipulation and control condition

Direct and vicarious contact

The manipulation of direct and vicarious contact did not differ from Experiment 2. The only difference between the two experiments was that once the two participants were briefed

about the task, they were immediately given a handout containing one measure asking how they felt at that very moment (see *Measures* section for more). More specifically, when the Greek Cypriot participant and the Turkish Cypriot confederate were informed about the task they needed to do, they were told that before they started with it they had to fill in one form each. The forms were placed on the table in front of them. They were asked to take a few minutes to complete the form and then turn it face down. The experimenter then left the room for them to complete the form and went to place the second participant in the second room. For this experiment, the second participant was placed in a room that was linked to the first room via a monitoring system. The vicarious contact participant got to listen to and watch the conversation of her friend with the Turkish Cypriot confederate via the monitoring system. After explaining to the second participant what her task was the experimenter asked the participant to complete the same short form the other two individuals were completing. After a few moments the experimenter reentered the first room to ask the Greek Cypriot participant and the confederate to start the task.

Imagined contact

When the participants came to the lab they were seated in a dimly lit booth which consisted of one desk with a computer screen and a keyboard, a head set, and two sheets of paper. Participants were told that they were required to imagine a scene that was described on the paper they had in front of them. They were told that they would be given one minute to imagine the scene, and that they should try to imagine it as vividly as possible. They were also told that in order to facilitate the mental imagery task, the experimenter would turn off the lights thus leaving the room very dark, and that they could wear the headset for any noise to be cancelled out. Then the experimenter gave them the paper and went through the scenario

with them. The scenario was in line with the elaborated imagined contact scenario proposed by Husnu and Crisp (2010 a, b) and read as follows:

“I will give you one minute to close your eyes and imagine yourself sitting at a café in Ledras street one Thursday afternoon. There you meet a Turkish Cypriot girl for the first time and a conversation begins. Throughout the conversation you find out many interesting and unexpected things about her.”

Upon having slowly read the scenario out loud, the experimenter asked the participant to respond to a questionnaire asking how the participants felt right then (see *Measures* section for more). Then the experimenter gave the participant a few moments to have another look at the scenario before reminding her that she should try to imagine the scene as vividly as possible. She then switched off the lights and closed the door and waited outside the room for the duration of one minute. When the time was up, the experimenter reentered the room, turned on the computer screen, and asked the participant to complete a questionnaire. A question preceded the actual posttest questionnaire, requesting the participants to describe in as much detail as they wished the scene they had imagined. This was done for purposes of strengthening the imagined contact intervention (see Crisp et al., 2008).

Control condition

The ‘online task’ the control participants were told they were going to do as part of the second phase of the study was essentially the completion of the posttest questionnaire. The only difference between Experiment 3 and Experiment 2 in how the control participants were treated was that in Experiment 3 the control participants were asked at the beginning of the

posttest questionnaire whether they happened to have had an interaction with (a) Turkish Cypriot/s over the past week (i.e., between pretest and posttest). The question had as follows: “In these past seven days, did you have the chance to meet any Turkish Cypriots?”. The scale ranked from 1, *no, none* to 5, *yes, quite a few*.

Measures

The majority of the measures were identical to the measures used in Experiment 2. There were, however, some notable differences which I list here.

a) *Prior intergroup contact*: One item asked participants: “How much contact have you had to date with Turkish Cypriots” (1, *no contact at all*; 5, *a lot of contact*).

b) *Positive action tendencies*: This measure consisted of four items adapted from Mackie, Devos, and Smith (2000). The participants were asked to answer on a five-point scale (1, *not at all*; 5, *very much*), to what extent they would: 1) “exchange visits with a member of the Turkish Cypriot community”, 2) “have a Turkish Cypriot friend”, 3) “accept a Turkish Cypriot as a family member”, 4) “meet more Turkish Cypriots” if they had the opportunity to do so.

c) *Pre task anxiety*: The exact same measure that was used for state intergroup anxiety was given to participants prior to the task. The instruction read: “Please circle the number that describes best how you are feeling **RIGHT NOW**”. The scale ranged from 1, *not at all* to 5, *very much*.

d) *Inclusion of other in self*: One item asked participants the following: “In the pictures below, imagine that one circle stands for you and one stands for the Turkish Cypriot you just had a conversation with. Please circle the picture that best describes how close to her you felt”. The wording was adjusted accordingly for the vicarious and the imagined contact

conditions. There were seven sets of circles with the first set showing no overlap between the circles and the last set showing full overlap between the two circles. Therefore the scale ranged from 1, *not close at all* to 7, *very close*.

e) *Delayed measure of attitudes towards outgroup member*: attitudes toward the confederate were repeated at delayed test. The measure used was the same (feeling thermometer) and the instruction asked participants to rate their feelings towards: “the individual you had a conversation with a week earlier”, for the direct contact participants; “the individual who your friend had a conversation with last week”, for vicarious contact participants; “the individual you imagined having an interaction with last week”, for imagined contact participants.

Design

The design utilized was a pretest-posttest-delayed posttest randomized design with three experimental conditions (direct contact condition, vicarious contact condition, and imagined contact condition) and a control condition.

Results

The plan of analyses followed in the previous experiments was adopted here too. I first assessed the random allocation of participants to the four conditions, using prior intergroup contact (mere contact and outgroup friendships) and the pretest scores on outgroup attitudes, intergroup anxiety, and willingness for contact as criterion variables. Secondly, I tested for differences between the three experimental conditions in terms of how the participants in each condition perceived the interaction in order to test Hypotheses 1 a-e. Thirdly, I checked for pretest-posttest, posttest-delayed posttest, and pretest-delayed posttest changes on the three

dependent variables within each condition (Hypotheses 2, 3, 4 and 8a) before comparing the posttest and delayed posttest effects of the four conditions (Hypotheses 5, 6, 7, and 8b).

Random allocation

The means and standard deviations for the variables on which the participants in the four conditions were compared to assess random allocation are given in *Table 3.5*. Participants reported overall almost no outgroup friendships ($M = 1.27$, $SD = .65$) and very little contact in general ($M = 1.72$, $SD = .98$). In fact, of the 75 participants, 41 reported having had no contact at all with the outgroup and 21 reported having had very little contact in the past. Their reported outgroup attitudes at pretest were more towards cold ($M = 4.59$, $SD = 2.01$) and their anxiety for intergroup interactions with Turkish Cypriots was fairly high ($M = 2.97$, $SD = .74$).

Table 3.5. Means and standard deviations for all conditions on the variables used to test random allocation

	Direct Contact <i>M (SD)</i>	Vicarious contact <i>M (SD)</i>	Imagined Contact <i>M (SD)</i>	Control Group <i>M (SD)</i>
Prior direct friendships	1.22 (0.55)	1.42 (0.84)	1.05 (0.22)	1.41 (0.80)
Prior direct contact	1.67 (0.91)	2.00 (1.29)	1.40 (0.60)	1.76 (0.97)
Pretest outgroup attitudes	4.94 (1.51)	4.16 (2.46)	4.85 (1.23)	4.29 (2.66)
Pretest intergroup anxiety	3.00 (0.78)	3.28 (0.81)	3.07 (0.61)	2.55 (0.60)
Pretest action tendencies	2.83 (0.87)	2.47 (1.14)	2.51 (0.93)	2.65 (1.19)

Multivariate Analyses of Variance (MANOVA) using Pillai's test, with Condition as the independent variable and the rest of the variables as dependent variables yielded a

marginal multivariate effect for condition ($V = .99$, $F(15, 204) = 1.58$, $p = .08$, partial $\eta^2 = .10$). Based on the new p -value (p -value = .01) which was corrected for multiple comparisons, none of the univariate effects was significant thus showing that random allocation to the four conditions was successful.

How participants perceived contact (Hypothesis 1)

Table 3.6. presents the means and standard deviations for the three experimental conditions for the variables on which they were compared, i.e., attitudes toward the outgroup member, group salience, inclusion of other in self, and pre-contact anxiety. The means and standard deviations of state intergroup anxiety are also provided. In order to check Hypotheses 1 a-d, I conducted one MANOVA using Pillai's trace with Condition as the independent variable and attitudes toward the outgroup member, group salience, inclusion of other in self, and pre-contact anxiety as the dependent variables. This analysis yielded a significant multivariate effect for Condition ($V = .99$, $F(8, 104) = 3.30$, $p < .01$, partial $\eta^2 = .20$). Three of the univariate effects were significant based on the new p -value ($p = .012$) which was corrected for multiple comparisons. These effects were the effect of Condition on attitudes towards the (imagined) outgroup member ($F(2, 54) = 5.39$, $p = .007$, partial $\eta^2 = .17$), on group salience ($F(2, 54) = 5.43$, $p = .007$, partial $\eta^2 = .17$) and on the inclusion of other in self ($F(2, 54) = 4.97$, $p = .01$, partial $\eta^2 = .16$).

As I had specific hypotheses on what the relationships between the three conditions would be, I used planned contrasts to test these hypotheses but also consulted the post-hoc tests to get the full picture. I checked for polynomial contrasts whereby the three conditions would follow a linear trend. As the order in which I expected the conditions to rank varied on the three variables, I conducted the three univariate analyses that turned out to be significant

individually, this time setting the contrasts I wanted to test. For the first univariate analysis where the dependent variable was the attitude towards the confederate (see Hypothesis 1a) I expected direct contact to yield the most positive attitudes and to be followed by vicarious contact which would be followed by imagined contact. To test this trend I coded the variables in the order in which I expected them to rank (direct contact = 1, vicarious contact = 2, and imagined contact = 3) and then asked for polynomial contrasts. The results showed that there was a significant linear trend ($p < .01$) whereby, as expected, direct contact yielded the most positive attitudes towards the outgroup, followed by vicarious contact and then by imagined contact. Post hoc tests showed a significant difference between direct contact and vicarious contact ($p < .05$) and between direct contact and imagined contact ($p < .05$), but no significant differences between the indirect types of contact ($p = 1.00$).

Table 3.6. Means and standard deviations across the three experimental conditions on the interaction-related variables

	Direct Contact	Vicarious contact	Imagined Contact
	<i>M (SD)</i>	<i>M (SD)</i>	<i>M (SD)</i>
Attitudes towards outgroup member	9.00 (1.41)	7.56 (2.13)	7.45 (1.28)
Group salience	1.70 (0.84)	2.13 (1.31)	2.80 (0.98)
Inclusion of other in self	4.89 (1.49)	3.33 (1.61)	3.95 (1.47)
Pre-contact anxiety	2.00 (0.71)	2.11 (0.89)	2.08 (0.63)
State intergroup anxiety	1.60 (0.71)	1.86 (0.71)	2.03 (0.65)

The same procedure was followed for the second univariate analysis where group salience was the dependent variable. In this case the conditions were ordered as follows:

imagined contact = 1, vicarious contact = 2, direct contact = 3, as I expected imagined contact to yield the greatest group salience, and to be followed by vicarious contact and then direct contact. The linear contrasts were significant ($p < .01$) thus providing support to Hypothesis 1b. Post hoc tests revealed a significant difference between imagined contact and direct contact ($p < .01$), but only a weak trend for differences between imagined and vicarious contact ($p = .16$). No differences occurred between direct and vicarious contact ($p = .85$).

For the last univariate analysis where inclusion of other in self was the dependent variable I expected, once again, a linear trend to describe the relationship between the three variables. I expected direct contact to lead to the highest inclusion of other in self and be followed by imagined contact and then vicarious contact. For this purpose the three conditions were ordered as such: direct contact = 1, imagined contact = 2, vicarious contact = 3. The linear trend was significant ($p < .01$) in this case too, hence Hypothesis 1c was supported. Post hoc analyses yielded significant differences between direct contact and vicarious contact ($p < .05$), a trend for significant differences between direct and imagined contact ($p = .17$) but no differences between imagined and vicarious contact ($p = .65$).

As the univariate effect for pre-contact anxiety was not significant this indicated that contrary to Hypothesis 1d, there were no differences across the three contact types with regards to the anxiety they elicited. To test Hypothesis 1e, I conducted a repeated MANOVA with Condition (direct vs. vicarious vs. imagined contact condition) as the between-subjects variable and Time of Measurement (pre-contact anxiety vs. state intergroup anxiety) as the within-subjects variable. There was a main effect of Time of Measurement ($F(1, 67) = 26.14$, $p < .001$, partial $\eta^2 = .28$). Pairwise comparisons revealed, in line with Hypothesis 1e, a significant pre-contact anxiety – state intergroup anxiety effect for direct contact ($p < .001$),

with participants in this condition reporting being more anxious before the contact than during contact. Unlike what was expected though, the same difference occurred for vicarious contact participants ($p < .01$), who also reported significantly more anxiety before observing the interaction than what they felt while observing the interaction. No significant effects were registered for imagined contact ($p = .22$).

Pretest - posttest - delayed posttest changes

In order to address Hypotheses 2, 3, 4, and 8a I conducted three repeated ANOVAs, one for each dependent variable where Condition (direct contact vs. vicarious contact vs. imagined contact vs. control condition) was the between-subjects variable and Time of Measurement (pretest vs. posttest vs. delayed posttest) the within-subjects variable. The means and standard deviations (SDs) across the three measurements and for each condition are given in *Table 3.7*. I asked for pairwise comparisons to check for pretest-posttest, posttest-delayed posttest, and pretest-delayed posttest changes within each condition. The results are presented in order for the three dependent variables.

Outgroup attitudes (Hypothesis 2)

The repeated measures ANOVA yielded a main effect of Time of Measurement ($F(2, 142) = 5.86, p < .01, \text{partial } \eta^2 = .08$). Pairwise comparisons yielded significant pretest-posttest changes *only* for the direct contact condition ($p < .01$) for which attitudes at posttest were more favourable than pretest attitudes. Even though pretest-posttest changes on outgroup attitudes were expected for vicarious and imagined contact too (see Hypothesis 2a) there was only a weak trend for more favourable attitudes at posttest for the vicarious contact condition ($p = .11$) and no effect for imagined contact ($p = .55$). Attitudes for the control condition as expected remained undifferentiated between pretest and posttest ($p = 1.00$). Hypothesis 2b

predicted attitudes to worsen between posttest and delayed posttest for all the contact conditions. Given that pretest-posttest differences only occurred for direct contact, Hypothesis 2b as it was formulated above only applied for this condition. In line with Hypothesis 2b, a significant posttest-delayed posttest change was observed for direct contact ($p < .05$) with attitudes becoming worse at delayed posttest in comparison to posttest. Attitudes remained undifferentiated between posttest and delayed posttest for the remaining conditions (vicarious contact: $p = 1.00$; imagined contact: $p = 1.00$; control condition: $p = .58$). According to Hypothesis 2c delayed posttest attitudes would not differ from pretest attitudes. Results were in agreement with Hypothesis 2c as no pretest-delayed posttest test differences were detected for the contact conditions (direct contact: $p = 1.00$; vicarious contact: $p = .55$; imagined contact: $p = .86$) and the control condition ($p = 1.00$). So, at delayed posttest outgroup attitudes were for all conditions undifferentiated in comparison to participants' pretest attitudes.

Intergroup Anxiety (Hypothesis 3)

The repeated measures ANOVA yielded a main effect of Time of Measurement ($F(2, 142) = 9.52, p < .001$, partial $\eta^2 = .12$). Pretest-posttest changes in intergroup anxiety were expected for all contact conditions but not the control group (see Hypothesis 3a). Pairwise comparisons revealed, partly in line with Hypothesis 3a, significant pretest-posttest changes for direct ($p < .001$) and for imagined ($p < .01$) contact but not for vicarious contact ($p = .56$). Intergroup anxiety reduced at posttest for direct and imagined contact. In contrast with Hypothesis 3a too, was the significant pretest-posttest change in the control condition ($p < .05$) according to which, however, *higher* intergroup anxiety was reported at posttest in comparison to pretest. Hypothesis 3b predicted that no changes would occur for intergroup

anxiety levels between posttest and delayed posttest in any of the conditions. The results gave support to Hypothesis 3b as there were no posttest-delayed posttest differences for any of the conditions (direct contact: $p = .68$; vicarious contact: $p = .43$; imagined contact: $p = .52$; control: $p = .67$). As for the pretest-delayed posttest comparisons, results were in line with Hypothesis 3c which expected delayed posttest anxiety levels to be lower than pretest anxiety levels. Pairwise comparisons indicated significant pretest-delayed posttest changes for direct ($p < .001$), imagined ($p < .05$) and vicarious ($p < .01$) contact. The anxiety reported by the participants in *all* contact conditions at delayed posttest was lower than pretest anxiety. In line also with Hypothesis 3c, was the null effect for the pretest-delayed posttest comparison for the control condition ($p = .15$).

Action tendencies (Hypothesis 4)

The repeated measures ANOVA yielded a main effect of Time of Measurement ($F(2, 142) = 7.07, p < .01, \text{partial } \eta^2 = .09$). I anticipated to find a rise in positive action tendencies at posttest for all contact conditions but not the control group (Hypothesis 4a). Pairwise comparisons revealed, partly in line with Hypothesis 4a, significant pretest-posttest changes for direct ($p < .05$) and for imagined ($p < .05$) contact but only a weak trend for vicarious contact ($p = .13$). In all cases action tendencies were more positive at posttest than in pretest. As expected no differences occurred for the control condition ($p = 1.00$). With regards to the posttest-delayed posttest comparisons, pairwise comparisons revealed significant changes for vicarious ($p < .05$) and imagined ($p < .05$) contact conditions with the delayed posttest action tendencies being *less positive* than posttest action tendencies. These findings were not in line with Hypothesis 4b which expected no differences between posttest and delayed posttest on this measure. On the other hand, in accordance with Hypothesis 4b, direct contact and control

condition yielded no posttest-delayed posttest changes (direct contact: $p = 1.00$; control condition: $p = 1.00$).

Lastly, the pretest-delayed posttest comparisons yielded only one marginal difference for the direct contact condition ($p = .09$) with the delayed action tendencies being more positive than the pretest action tendencies, as predicted by Hypothesis 4c. In contrast to Hypothesis 4c though, the delayed posttest scores were not different from the pretest scores for vicarious ($p = 1.00$) and imagined contact ($p = 1.00$). No pretest-delayed posttest differences were found, as was expected, for the control condition ($p = 1.00$).

Attitudes towards the confederate (Hypothesis 8a)

The repeated measures ANOVA yielded no effect of Time of Measurement ($F(1, 55) = .26$, $p = .61$, partial $\eta^2 = .01$). None of the pairwise comparisons was significant. As expected (see Hypothesis 8a), the attitudes towards the confederate, unlike outgroup attitudes, remained undifferentiated and therefore equally favourable from posttest to delayed posttest. This was the case for all contact conditions.

Differences across conditions

As in Experiment 2, I conducted two ANCOVAs per dependent variable to assess whether there were differences across conditions on their posttest and delayed posttest scores on the three dependent variables (outgroup attitudes, intergroup anxiety, and willingness for contact). One ANCOVA was conducted also to determine whether there were differences across the contact conditions on the delayed attitudes towards the outgroup. When the effect of Condition was significant I looked into simple contrasts whereby each contact condition was compared to the control condition. Given that these are non-orthogonal contrasts I adopted a new p -value which was corrected for multiple comparisons. Post-hoc tests were also

consulted to look for possible differences between the contact conditions unless I had set hypotheses for the relationships between them in which case the appropriate contrasts were used. The results of these analyses are reported for each dependent variable separately.

Outgroup attitudes (Hypothesis 5)

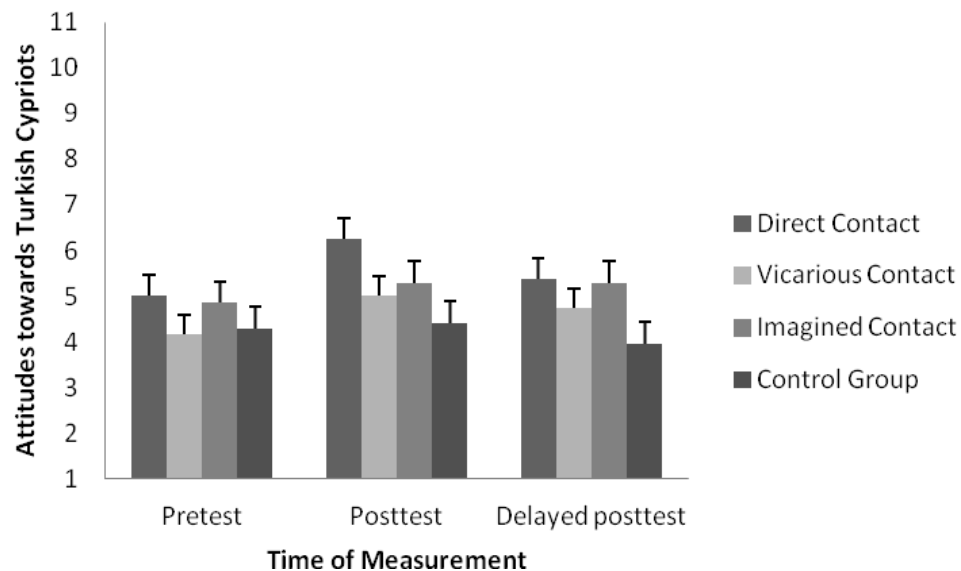
Figure 3.3. portrays the pretest, posttest, delayed posttest scores on outgroup attitudes for each condition. For the first ANCOVA the interaction term between Condition and Pretest attitude scores was non-significant ($F(3, 86) = .45, p = .72$) indicating that the assumption of the homogeneity of regression slopes held. The ANCOVA yielded a marginal main effect for Condition ($F(3, 70) = 2.70, p = .05$, partial $\eta^2 = .10$). Simple contrasts⁵¹ indicated in line with Hypothesis 5a (according to which all contact conditions would lead to significantly more positive outgroup attitudes in comparison to the control condition), significant differences between direct contact and the control condition ($p = .007$). Direct contact participants reported more positive outgroup attitudes at posttest in comparison to control condition participants. Unlike what was expected by Hypothesis 5a there were no significant differences between the vicarious contact condition and the control condition ($p = .19$) and the imagined contact condition and the control condition ($p = .27$). To test Hypothesis 5b, i.e., that direct contact would yield the most positive posttest outgroup attitudes followed by vicarious contact, followed by imagined contact, leaving the control condition last, polynomial (linear) contrasts were tested. For this purpose the four conditions were coded as follows: direct contact = 1, vicarious contact = 2, imagined contact = 3, control condition = 4. The linear polynomial contrasts were significant ($p < .01$) thus providing support to Hypothesis 5b. There were, however, no differences in the posttest outgroup attitudes between direct and vicarious contact conditions ($p = .77$), a finding which was in line with the null effect found

⁵¹ As simple contrasts are non-orthogonal a more conservative p -value ($p = .017$) was adopted.

in Experiment 2, and no differences between direct and imagined contact condition either ($p = .47$).

The second analysis showed a non-significant interaction between Condition and Pretest scores ($F(2, 69) = .82, p = .45$) which meant that the assumption of homogeneity of the regression slopes held for this analysis. There was no effect for Condition on attitudes at the delayed posttest ($F(3, 70) = 1.44, p = .24$, partial $\eta^2 = .06$) thus indicating that there were no differences across conditions with regards to their delayed posttest outgroup attitudes. In other words even the posttest effects of (direct) contact on attitudes had faded away and so the differences between direct contact and the control condition disappeared one week after the intervention. This finding was in line with Hypothesis 5c.

Figure 3.3. Pretest – posttest – delayed posttest scores of each condition on outgroup attitudes

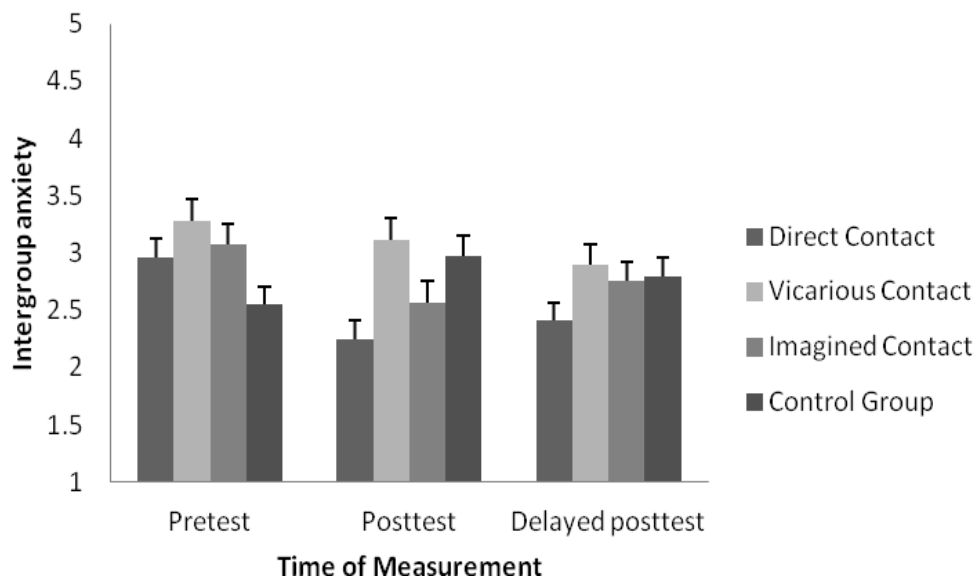


Intergroup anxiety (Hypothesis 6)

Figure 3.4. presents the pretest, posttest, and delayed posttest means of each condition on intergroup anxiety. For the first ANCOVA the interaction between Condition and Pretest

scores on intergroup anxiety was non-significant ($F(3, 67) = .19, p = .90$) thus showing that the homogeneity of regression slopes assumption was in place. Hypothesis 6a predicted that posttest anxiety levels would be significantly lower for the three contact conditions in comparison to the control condition. In line with Hypothesis 6a, the ANCOVA yielded a significant effect for Condition ($F(3, 70) = 11.65, p < .001$, partial $\eta^2 = .33$) and simple contrasts yielded significant differences between direct contact and the control condition ($p < .001$), and the imagined contact and the control condition ($p < .001$) as well as marginal differences for vicarious contact and the control condition ($p = .02$). In all cases the contact conditions led to less intergroup anxiety at posttest in comparison to the control condition.

Figure 3.4. Pretest – posttest – delayed posttest scores of each condition on intergroup anxiety



The interaction between Condition and Pretest scores on intergroup anxiety for the second ANCOVA, was not significant ($F(3, 67) = 1.98, p = .13$) and therefore the assumption of homogeneity of regression slopes was met. Hypothesis 6b expected the delayed posttest intergroup anxiety to be significantly lower in the contact conditions in comparison to the

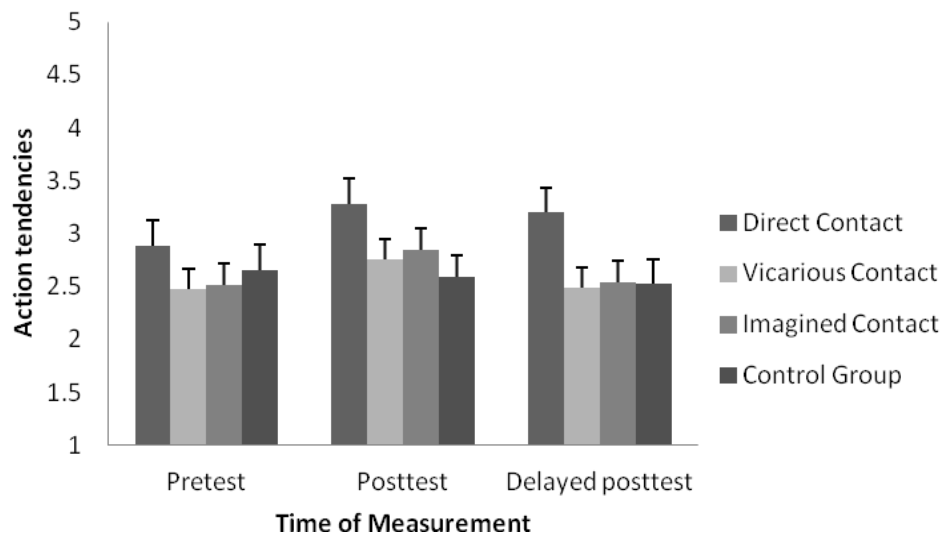
control condition. In line with Hypothesis 6b, the analysis yielded a significant effect for Condition ($F(3, 70) = 6.26, p < .01$, partial $\eta^2 = .21$) and simple contrasts showed significant differences between *all* contact conditions and the control condition (direct contact vs. control: $p < .001$; vicarious contact vs. control: $p = .005$; imagined contact vs. control: $p = .006$). Finally, in full agreement with Hypothesis 6c, no differences were detected across the contact conditions in either the posttest or the delayed posttest.

Action tendencies (Hypothesis 7)

Figure 3.5. presents the pretest, posttest, and delayed posttest means of each condition on action tendencies. For the first ANCOVA the interaction between Condition and Pretest scores on action tendencies was non-significant ($F(3, 67) = .14, p = .94$) thus showing that the homogeneity of regression slopes assumption was met. Hypothesis 7a anticipated all contact conditions to yield significantly more positive action tendencies at posttest in comparison to the control condition. The ANCOVA yielded a significant effect for Condition ($F(3, 70) = 3.55, p < .05$, partial $\eta^2 = .13$) but simple contrasts revealed a significant difference only between direct contact and the control condition ($p < .01$). There was a weak trend for a difference between vicarious contact and the control condition ($p = .09$), and a marginal effect for imagined contact ($p = .02$). In order to test Hypothesis 7b, according to which direct contact would yield the most positive action tendencies at posttest, that imagined contact would follow and vicarious contact would come third, leaving the control condition last, the linear polynomial contrasts were tested. For this purpose, the four conditions were coded as follows: direct contact = 1, imagined contact = 2, vicarious contact = 3, control condition = 4. The linear polynomial contrasts were significant ($p < .01$) and therefore Hypothesis 7b was

confirmed. Post-hoc analyses, however, showed no significant differences between any two types of contact.

Figure 3.5. Pretest – posttest – delayed posttest scores of each condition on action tendencies



The interaction between Condition and Pretest scores on delayed action tendencies for the second ANCOVA, was not significant ($F(3, 67) = .14, p = .95$), hence the assumption of homogeneity of regression slopes was confirmed. I expected direct and perhaps imagined contact to lead to significantly more positive action tendencies at delayed posttest in comparison to the control condition (Hypothesis 7c). The analysis did yield a significant effect for Condition ($F(3, 70) = 3.08, p < .05$, partial $\eta^2 = .12$) but a look at the simple contrasts showed only a significant difference between direct contact and the control condition ($p < .01$) but no difference between imagined contact and control condition ($p = .58$). Vicarious contact's effects at delayed posttest too were undifferentiated from the control condition's effects ($p = .67$). Post-hoc comparison furthermore showed a weak trend for different delayed

action tendencies between direct contact and vicarious contact ($p = .13$) and between direct and imagined contact ($p = .17$).

Attitudes towards outgroup (Hypothesis 8b)

The effect of the Condition by the Covariate (initial attitudes towards confederate) interaction was not significant ($F(2, 52) = .76, p = .47$), so the assumption of homogeneity of regression slopes was met. The ANCOVA did not yield any effect for the Condition ($F(2, 54) = .02, p = .98$, partial $\eta^2 = .00$) thus showing that the three types of contact did not differ at all with regards to their attitudes towards the (imagined) outgroup a week after the intervention. This finding was not in agreement with Hypothesis 8b which anticipated direct and vicarious contact to yield even more positive attitudes towards the outgroup in comparison to imagined contact.

Discussion

The results of Experiment 3 will be discussed mainly in relation to Experiment 2 as the two studies employed the exact same experimental design (pretest-posttest-delayed posttest with a control condition). References to the results of Experiment 1 will also be made. In Experiment 3, I strived to replicate the results of Experiment 2 as well as elaborate on it. This was done in four important ways: 1) I included one more type of intergroup contact, imagined contact, the effects of which I compared against the control condition as well as the other two contact conditions, 2) I included one more dependent variable, namely action tendencies, predominantly because I wanted to assess (and compare) the effect of the two indirect types of contact on this variable, 3) I included one more measure of contact-related anxiety which I called pre-contact anxiety that aimed to measure the anxiety of participants upon hearing that they were required to engage in an intergroup interaction of some sort, 4) I measured

participants' attitudes towards the outgroup member (Turkish Cypriot confederate/ imagined outgroup member) a week after the intervention took place to see whether the attitudes towards the single member unlike the attitudes towards the whole outgroup can have more lasting effects, 5) I included one more measure of prior intergroup contact that did not tap friendships but asked about the general levels of contact the participants had with the outgroup so as to better understand the background of my participants in terms of prior intergroup experiences, and 6) I tried to employ a more 'compatible' control condition by asking participants in the control condition to *think* about intergroup contact before completing the posttest measures instead of doing nothing at all (as in Experiment 2).

The results of Experiments 1 and 2 with regards to the immediate effects of contact on outgroup attitudes were only partially replicated: direct contact led to significantly more positive attitudes at posttest, whereas only a trend for more positive attitudes was detected for vicarious contact this time around. The comparison with the control condition showed only direct contact to yield significantly more positive attitudes at posttest whereas vicarious contact and control condition were undifferentiated. Any comparisons between the results of Experiments 2 and 3 on outgroup attitudes though should be made with caution, because in Experiment 2 the attitudes reported by the control participants followed an unexpected course through time by becoming less favourable at every subsequent measurement. In Experiment 3 on the other hand attitudes remained unchanged in time for the control condition, so in some ways the control condition in Experiment 3 formed a stricter point of comparison for the effects of contact on outgroup attitudes.

Imagined contact too did not yield more positive attitudes at posttest in comparison to the control condition. Unlike the other two types of contact, imagined contact did not even

yield pretest-posttest changes. Even though this null finding for imagined contact is not in agreement with previous findings of imagined-contact studies (e.g., Turner et al., 2007c), it may not come as a complete surprise to imagined contact researchers. In their review article, Crisp et al. (2008) admitted that imagined contact's indirect nature would render its effects on attitudes weaker than the effect of direct contact on attitudes.

Despite the fact that the results of Experiment 3 speak in favor of the greater effectiveness of direct contact in yielding more favourable outgroup attitudes, at least in the short-term, in comparison to the other two contact conditions this claim is not fully supported as the outgroup attitudes reported by the participants in the direct contact condition at posttest were not significantly more favourable than the attitudes reported by the participants in the other two contact conditions. Additionally, a common finding between Experiments 2 and 3 was that attitudes reverted back to their original scores at the delayed posttest. One week after the interaction took place any contact effects that occurred on outgroup attitudes just after the interaction (i.e., at posttest), disappeared. The replication of this finding reinforces the suggestion made earlier that a single interaction with the outgroup does not seem to suffice for a more permanent change in outgroup attitudes regardless of the type of contact employed.

In this experiment I included a delayed measure on the attitudes of the participants towards the outgroup with whom they interacted. I expected that unlike outgroup attitudes that got worse a week after the interaction; the attitudes towards the individual outgroup member would remain at the same levels. In line with my hypothesis, attitudes towards the outgroup member did not change from posttest to delayed posttest for any of the contact types. Since attitudes towards the outgroup member were largely positive after contact, the fact that these attitudes remained positive a week after contact is certainly an encouraging and promising finding for

intergroup contact as a whole. It is worth pointing out here that while direct contact produced more favourable initial attitudes towards the outgroup than both vicarious and imagined contact, there were no differences across conditions at the delayed measure of attitudes towards the outgroup individual. Attitudes towards the outgroup were equally positive across conditions at delayed posttest.

The results of this experiment regarding intergroup anxiety were positive for intergroup contact and particularly the indirect types of contact. All types of contact led to a decrease in intergroup anxiety that was evident at posttest already for direct and imagined contact and emerged at delayed posttest for vicarious contact. In any case, the participants in the three contact conditions reported significantly lower intergroup anxiety in comparison to the participants in the control condition at the delayed posttest thus showing that anxiety reduced after contact but also remained lower a week after the contact had taken place. It should be noted here that the control condition exhibited an unexpected course on the anxiety measure with the reported intergroup anxiety significantly rising at posttest but being undifferentiated from pretest levels at delayed posttest. So if nothing else, the finding that all contact types differed from control condition at the delayed posttest is a commendable and strong finding. This finding is largely in line with the findings of Experiment 2 on the same variable, even though in Experiment 2 the effects of direct contact on anxiety reduction were only marginal and its comparison to the control condition, non-significant. Consistent across the two experiments were the findings of vicarious contact on the other hand, whereas imagined contact successfully reduced anxiety for future contact too.

Importantly, the effects of both indirect forms of contact were significantly different from the control condition, but undifferentiated from the effects of direct contact thus

suggesting that at least with regards to intergroup anxiety indirect types of contact can be as effective as direct contact. It should also be noted here that the two indirect types of contact did not differ from each other on this variable. This finding is consistent with the contentions made according to which indirect types of contact may be less likely to match direct contact in terms of attitude change (see Crisp et al., 2008) but that their real contribution is found in preparing individuals for future contact. The reduction of the anxiety stemming from the thought of engaging in an intergroup interaction is certainly a good first step towards preparing one for participating in (see Levin et al., 2003) and for benefitting from (see Islam & Hewstone 1993; Stephan & Stephan, 1985) future intergroup interactions.

The findings on the measure of action tendencies which was only employed in the third experiment showed direct contact, as expected, to lead to significantly more positive action tendencies from pretest to posttest and this effect held over time. Direct contact differed significantly from control condition at posttest, and marginally at delayed posttest on this measure. Imagined contact, as expected, led to more positive action tendencies at posttest. This is a finding that replicates the positive effects of imagined contact on action tendencies in Cyprus, found by Husnu and Crisp (2010a) with Turkish Cypriots as their target group. However, the effects of imagined contact were not significantly different from the effects of the control condition on posttest action tendencies even though there was a strong tendency for differentiation present. Its effects were also short-lived as they faded away a week after contact. Vicarious contact on the other hand had no effects on action tendencies, a finding that comes as a stark contrast to vicarious contact's consistent effects on reducing anxiety for intergroup contact and in contrast also with results from previous studies showing that

vicarious contact can lead to more positive action tendencies (see Mazziotta et al., 2011, Experiment 1).

The results of Experiment 3 on action tendencies demonstrated an advantage of direct contact over indirect types of contact, and a slight advantage of imagined contact in comparison to vicarious contact in promoting more positive action tendencies. These findings suggest that at least for a group of people whose prior experience with the outgroup had been as minimal, or non-existent as those of the individuals participating in this experiment, indirect types of contact (particularly vicarious contact) may not be as successful as direct contact in yielding effects on behavioural intentions. The results also hint at one important difference between types of contact that include the self (i.e., direct and imagined contact) and types of contact where the self is not directly involved in the intergroup event (i.e., vicarious contact).

There may be something imperative in having the self performing either in real life or mentally a novel act that renders the individuals more likely to see themselves being involved in an intergroup contact situation again. On the other hand, seeing someone else performing a novel act might aid one to evaluate the intergroup interaction as such (which is related to Wright et al.'s, 1997, argument about extended contact having an influence on ingroup and outgroup norms), as opposed to aiding one to seeing oneself being part of this kind of interaction in the future. In other words, whereas for the direct and imagined contact the focus is on the self, and more particularly the self as an actor in an intergroup incident, in the vicarious contact condition the emphasis is on the situation under observation more than the self. This argument derives some support by the finding that inclusion of other in self was shown in this experiment to be higher in the direct contact condition, followed by imagined

contact condition, and then vicarious contact. Perhaps this is also the reason that the effects of vicarious contact on intergroup anxiety only became traceable at the delayed posttest, presumably when vicarious contact participants had the time to put themselves in the picture and tell how they would feel if in an intergroup situation.

I would like to end the discussion of this chapter by commenting on the findings regarding the measures tapping the interaction, some of which were very intriguing. The participants in the three conditions differed along three out of the four variables they were compared on. The first two were attitudes towards the outgroup and inclusion of other in self and they were discussed earlier on. The third variable the three conditions differed on was group salience. Imagined contact triggered the highest group salience, and was significantly higher than the direct contact condition and marginally higher than the vicarious contact condition. This finding becomes more interesting if seen in combination with the levels of state intergroup anxiety. Even though the three types of contact were not compared on this measure, a look at the means informs us that state intergroup anxiety reported by imagined contact participants seems higher than state intergroup anxiety reported by direct contact participants⁵². Direct contact on the other hand yielded the lowest group salience. If group salience is a source of anxiety then the differences in state intergroup anxiety between the two conditions (direct and imagined contact) could be attributed to the differential levels of group salience experienced at contact. I conducted a mediation analysis using bootstrapping to test this assumption. The indirect effect of condition ('direct contact' = -1, 'imagined contact' = +1) on state intergroup anxiety was significant at the 0.01 level (point estimate: 0.17 with a 99%

⁵² A univariate ANOVA comparing the three contact conditions on state intergroup anxiety was not significant, $F(2, 55) = 1.97, p = 0.15$, but did show a trend for differences. A look at the simple contrasts comparing direct and extended contact with imagined contact on this measure, yielded a marginal effect for the comparison between direct and imagined contact ($p = .06$)

confidence interval of [.01, .49] thus giving support to my hypothesis that the higher group salience reported by imagined contact participants (as opposed to direct contact participants) explained the higher anxiety experienced at contact by imagined contact participants

Interestingly, while participants in the three contact conditions did not differ at all in terms of their reported pre-contact anxiety, for direct and vicarious contact conditions pre-contact anxiety was higher than the anxiety the participants reported having experienced during contact. This finding at least concerning direct contact is very much in line with recent results from a series of studies by Mallett, Wilson, and Gilbert (2008) who demonstrated that intergroup encounters turn out to be more pleasant than people expect them to be. The discrepancy between pre-contact anxiety and experienced anxiety in direct and vicarious contact conditions could be attributed to the relatively low group salience reported by the participants in the two conditions. To have been able to test this assumption I should have included another measure of group salience (pre-contact group salience) to assess whether group salience before contact was higher than the group salience reported after contact and whether these changes in group salience affected anxiety.

Another possibility is that the reported state intergroup anxiety was affected by cognitive dissonance experienced by the participants in the direct and vicarious contact conditions. Participants in these conditions might have ended up reporting very low anxiety at the time of the interaction (lower than their anxiety before the interaction) not because they actually felt less anxiety throughout the interaction but because they thought that since the interaction went well and was positive then they must have been in a non-negative state themselves.

It is interesting furthermore to note that this discrepancy between pre-contact anxiety and state intergroup anxiety was noted in the two cases (direct and vicarious contact) where the participants experienced an *actual* intergroup interaction and not when the participants imagined this interaction. Despite that, imagined contact participants just like participants in the other two contact conditions reported having lower intergroup anxiety for future intergroup encounters after the interaction, an effect that also lasted in time. So if there is any value in the change between pre-contact anxiety and state intergroup anxiety in changing individuals' anxiety for future intergroup interaction, this may apply only in those cases in which individuals are exposed to an actual intergroup situation.

General Discussion

The importance of alternative types of contact has been repeatedly stressed, and their potential benefits especially in segregated contexts have been outlined. We know by now that both direct and indirect types of contact can lead to positive changes in prejudice-related outcomes but what formed a gap in the literature was how these new types of (indirect) contact fared in comparison to face-to-face contact in terms of the *breadth*, the *magnitude*, and the *longevity* of their effects. There have been furthermore no studies comparing the prejudice-reduction effects of the different indirect types of contact with each other (e.g., vicarious vs. imagined contact) even though this sort of comparison has been alluded to. Lastly, very few studies in the literature of intergroup contact have actually looked into the contact event itself to try to understand what happens during the interaction. These lapses in the literature were identified and addressed by the three experiments reported in this chapter. I will now briefly summarize the results of the three experiments and draw general conclusions

based on the findings. I will end the general discussion section with some weaknesses of these studies and suggestions for future research.

I should reiterate before summarizing the findings of the three experiments that prior contact of the participants in these studies was found to be scarce and as the measure of ‘mere contact’ in Experiment 3 revealed, the majority of the participants had not in any way interacted with an outgroup member in the past. Although the low levels of contact are not a surprise in this context they should be taken into consideration when interpreting the results of these experiments.

The effects brought about by vicarious and direct contact were compared in Experiment 1, and against a control condition in Experiments 2 and 3, and the effects of imagined contact were tested against the other two types of contact (and a control condition) in Experiment 3. As far as attitudes towards Turkish Cypriots are concerned, the results showed that direct and vicarious contact but not imagined contact caused more favourable attitudes towards Turkish Cypriots at posttest (an effect that was weak for vicarious contact in the third study). Even though there was some evidence for direct contact’s ability to produce even more profound changes in this measure in comparison to vicarious contact, this did not ultimately matter as outgroup attitudes in any case returned back to their initial (pretest) levels a week after contact. This was a consistent finding in Experiments 2 and 3 where a delayed posttest was introduced. To me, this result was an obvious indication that a single intergroup interaction with a single outgroup member cannot bring about permanent changes to outgroup attitudes. Furthermore, the change of attitudes from posttest to delayed posttest could also be reflecting a realization on behalf of the participants that the outgroup member they had interacted with was only a single member of the outgroup community. They might have

therefore reasoned that they could not or would not simply change their attitudes towards a whole group from merely having met one outgroup individual.

On the other hand, it is hopeful that attitudes towards the outgroup individual (as revealed in Experiment 3) did not change from posttest to delayed posttest for any of the contact conditions. This shows that even though changes towards the whole outgroup did not occur in the long-run (or in the short-run for imagined contact), the positive attitudes formed towards an outgroup member remained positive and they were not just a temporary artifact of a pleasant intergroup experience.

The three contact conditions were also compared in terms of the changes they caused on intergroup anxiety and action tendencies. These were the two variables that were of real interest for the indirect types of contact which were conceived as occurrences (or in this case, interventions) to be able to decrease individuals' apprehension about involving in intergroup encounters and to also increase their willingness to engage in them (see Crisp & Husnu, 2011). The effects of vicarious and imagined contact on intergroup anxiety provided strong support to these contentions. Vicarious and imagined contact led to lower anxiety which (at delayed posttest) was significantly lower than the anxiety levels reported by participants in the control condition and as low as the intergroup anxiety reported by the direct contact participants for whom a reduction in anxiety was also found.

The effects of vicarious contact on intergroup anxiety were particularly commendable both because of their consistency (intergroup anxiety was found to be reduced after the participants were vicariously exposed to the outgroup in both Experiments 2 and 3), but also because of the latency with which they emerged (i.e., at delayed posttest), in other words, a sleeper effect. I suggested earlier that this delay in the yield of the effects of vicarious contact

on intergroup anxiety may be due to the fact that vicarious contact participants did not immediately associate the situation they observed with themselves.

There were no effects on action tendencies for vicarious contact whereas the effects of imagined contact were not strong enough to be distinct from the control condition or to last in time. It is worth pointing out here that this was the first time that the effects of imagined contact were tested for their longevity. To my knowledge, none of the imagined contact studies published to date has included a delayed measurement so Experiment 3 formed a very difficult test for imagined contact. Despite that, there were pretest-posttest differences detected within the condition. For the participants in the imagined contact condition, posttest action tendencies were more positive than their pretest action tendencies. Direct contact, on the other hand, led to more positive action tendencies at posttest and delayed posttest in comparison to pretest and also in comparison to the control condition (even though the difference was marginal between the two conditions at delayed posttest).

I suggested in the discussion of Experiment 3 that the findings in action tendencies may point to possible differences between vicarious and imagined contact. Previous studies in imagined contact have underscored that the mental processes involved in imagining an intergroup interaction resemble the mental processes that are in place when the interaction is actually taking place (see Turner et al., 2013) and they also stressed that imagined contact shares one important similarity with direct contact which is the involvement of the self in the interaction (see Crisp & Husnu, 2011). In fact, Crisp and Husnu (2011) made an interesting point according to which “envisaging oneself taking part in a positive outgroup encounter can form the basis for contact-based perception” (p. 277). I believe that the results of this study with regards to action tendencies are to a great extent in line with these postulations of Crisp

and Husnu (2011). As noted also in the discussion of Experiment 3, the self was not the focus in the vicarious contact condition, hence observing an intergroup interaction did not contribute to forming the “basis for contact-based perception”. I reason that this explains why direct and to some extent imagined contact had effects on action tendencies unlike vicarious contact.

One may raise the question in this case about how the effect of vicarious contact on intergroup anxiety reduction can be explained given that intergroup anxiety is defined as *one's* feelings of anxiety when *one* finds *oneself* in an intergroup context. I think there is a subtle but crucial difference between hypothetically placing oneself in an intergroup scenario in order to see how one would feel, on one hand, and asking one whether one would ‘dare’ to take part in this sort of encounter, on the other hand. The results of these experiments show that while participants in the vicarious contact condition essentially report that they would feel more comfortable in an intergroup situation, direct and to a lesser extent imagined contact participants claim that they would pursue such an interaction themselves. Pursuing an intergroup interaction is different from coping with it, and in order to pursue an act, in my opinion, one needs to have what Crisp and Husnu (2011) called “an informational base – a behavioural script – upon which to make inferences about one’s own attitudes towards engaging in intergroup contact” (p. 277). This script is only available once one gets in the process of imagining or experiencing an intergroup encounter (where the self is the actor). I propose that vicarious contact is not as effective at getting someone there (i.e., in the position of attaining this script) compared with either direct or imagined contact.

One final point has to do with the fact that all three experiments included measures that aimed to look into the interaction. I judge this to be a very important contribution of this set of studies primarily because the contact event in the intergroup contact literature has been

treated somewhat as a black box, as Harwood (2010) puts it, into which “people disappear and emerge with different attitudes” (p. 164). Apart from serving as a look into the black box of contact, the measures on the interaction also served to test Wright et al.’s (1997) postulations that extended contact’s advantage over direct contact is that it elicits lower intergroup anxiety and higher group salience at the time of the interaction. These contentions were not supported by the findings of these studies. Perhaps this is because of the way vicarious contact was operationalised (observing a *friend* interacting with an outgroup member) as this may have decreased group salience for vicarious contact participants for whom their friend was first and foremost a familiar individual and not an ethnic ingroup member. Should this be the case, then a situation in which the participant in the vicarious contact condition observes an unknown ingroup member interacting with an outgroup member would, in theory yield higher group salience.

Anxiety also was very low in both conditions or at least it was thought/ reported to be very low by the participants. It is hard to tell whether anxiety as reported by the participants after contact reflected well how the participants actually felt during contact. As it is in any case hard to evaluate emotions through self-reports, observational (see Schlenker & Leary, 1982) or physiological measures (see, Blasovich, Mendes, Hunter, Lickel, Kowai-Bell, 2001; Page-Gould, Mendoza-Denton, & Tropp, 2008) could have shed light to that end. The striking finding, however, was the discrepancy between the anxiety the direct and vicarious contact participants reported having experienced upon being told that the task involved an intergroup interaction (pre-contact anxiety) and the anxiety they reported having experienced during the task (state intergroup anxiety). Once again even though self-reports can be misleading, this remains to be an important finding which opens the route for research akin to the research of Mallet et al. (2010) which tapped the differences between individuals’ expectations of a

contact event and their actual impressions of the event. It is furthermore interesting that this discrepancy only occurred in the two cases where the intergroup interaction took place in reality (i.e., direct and vicarious contact). It is an impossible task to attribute this discrepancy between pre-contact anxiety and state intergroup anxiety to a single cause and therefore more research is needed to eliminate some of the possible causes and give support to others.

Some limitations of the research

There are a few methodological weaknesses in these studies that should be acknowledged. To begin with, I chose to operationalize vicarious contact as ‘watching a friend interacting with an outgroup member’. I chose friends over unknown ingroup members because I reasoned that friends would be a valid and trustworthy source for participants in vicarious contact, as opposed to unknown ingroup members who could be classified as ‘exceptions to the rule’. Even though there is nothing wrong with the operationalisation per se, my concern is that I may have not captured some inter- or rather intra- personal elements that could have contributed to the way the participants in the vicarious contact condition behaved. For example seeing one’s friend engaging in a very intimate contact with a stranger (let alone the fact that the particular stranger was also an outgroup member) to whom she also disclosed some intimate information, might not have been seen with a positive eye by all members of the vicarious contact condition. To capture this kind of reaction of the participants in the vicarious contact conditions I should have included more questions tapping their reflections on watching their friends interacting with and/ or getting close to the outgroup member.

It may be worth bringing up here, that in some of the studies reported by Wright et al. (2008) the personal closeness of the ingroup member was manipulated and so in one condition the participants in the vicarious contact condition observed a friend interacting with the

outgroup, and in the other condition they observed an ingroup stranger interacting with an outgroup. The interactions were in one condition friendly, in the other neutral, and in the other hostile. The results showed that the participants in the vicarious contact condition were more affected by each encounter when their friend was the one interacting with the outgroup. So when participants observed a friend engaging in a friendly interaction with the outgroup this yielded the most positive evaluations of the outgroup, and watching a friend in a hostile intergroup interaction led to the most negative evaluations of the outgroup, whereas watching an ingroup stranger in these situations mattered less.

A second possible methodological drawback concerns task-demand characteristics and the likelihood that they might have been a confounding factor in these experiments. Despite the initial attempt to conceal the otherwise obvious focus of the studies, a valid criticism of this set of studies is that the results may simply have been an artifact of task-demand characteristics. In other words participants might have realized what the experimenter was after and thus gave desirable answers on the self-reports. Without wanting to reject this possibility out of hand, I would propose that in the same way that one would argue that task-demand characteristics in these studies pushed some participants to give answers that would be 'liked' by the experimenter, participants with very negative attitudes towards Turkish Cypriots could have reacted to the 'visible intentions' of the experimenter. If the latter were the case then I would expect to trace this negative reaction in the participants' scores but as far as I could see there were no posttest scores that would suggest that⁵³. Furthermore, if participants were interested in pleasing the experimenter then how can their delayed posttest

⁵³ By this I mean that a look at the pretest-posttest differences did not show a sharp decrease in intergroup attitudes or any other of the other measures that would suggest that there were really unhappy people in the group. Similarly a look at the ratings of the outgroup member (confederate/ imagined individual) shows that the vast majority of the participants (approximately 80% across all studies) ranked between 9-11 (i.e., 80 degrees to 100 degrees) and from those who rated the confederate lower the lowest was 50 degrees.

scores on attitudes be explained? If the participants wished to entertain the experimenter's wishes then they would have to do so across all measures at all time points. To further investigate the role of demand characteristics in experiments such as these, future research should ask participants to guess what the study was about, and if possible, use implicit measures of prejudice that are thought to be protected from task-demand characteristics (e.g., Turner & Crisp, 2009).

The third methodological problem is the use of solely female participants in these studies, who were chosen because of ease of access. These studies should be repeated with male participants. Greek Cypriot males need to serve a mandatory military service in the national guard of the Republic of Cyprus from the age of 18 for 24 months. I suspect that their attitudes towards Turkish Cypriots just after that (i.e., when they are at university) will be more negative than the attitudes of female Greek Cypriots and will also be more firm and rigid. I would, therefore expect different results when repeating these studies with males, but I cannot say with certainty what this difference will be. Studies conducted by Hodson (2008) and Dhont and Van Hiel (2009) have shown that intergroup contact does not only have the desirable effects but it is often even more effective amongst more intolerant and rigid individuals (see Hodson, 2011, for a review). It would be certainly interesting to see if these applied to Greek Cypriot males recently released from their military duties.

With regards to the experimental design, even though I utilized one of the strictest designs, a pretest-intervention-posttest-delayed posttest design with a control condition to test the effects of contact on prejudice-related measures, one could argue that the inclusion of a pretest made individuals more susceptible to the effects of the intervention. With this being a possible danger, the most appropriate design to have used perhaps would have been the

Solomon design which would nonetheless require double the number of conditions, i.e., the initial two, three, or four conditions as employed in Experiments 1, 2, and 3 respectively plus the same amount of conditions but without the pretest. In this way the effect of the pretest would be factored in. The difficulty with such a design, of course, lies in the number of the participants required but its strength over the design that was used in my experiments is not to be neglected.

Finally, for full compatibility with the direct and the vicarious contact conditions, I could have asked the participants of the imagined contact condition to imagine completing with an outgroup stranger a task similar to the Closeness Induction Task the direct participants had to complete with the Turkish Cypriot confederate or any task that would specifically require reciprocal self-disclosure (e.g., Pagotto et al., 2012) since self-disclosure was such a key aspect of the direct intergroup encounter.

In retrospect, I think that the control condition could have been handled in a different way too. I attempted what I thought to be an amendment to the treatment of the control condition in Experiment 3 where I asked participants to at least think of intergroup contact by asking them to report on how many intergroup contact experiences they had in the past week. I doubt though how much this encouraged them to think about intergroup experiences given that none of them had any intergroup experiences and the likelihood of having any was close to nil anyway. In contrast, I could have asked them to take a minute to think when was the last time they met a stranger who was not a Greek Cypriot and to report on this encounter.

Alternatively, I could have had the participants in the control condition come to the lab and have/ observe/ imagine an interaction with a stranger who was not an outgroup member. Any of these would have formed better control conditions.

Despite the aforementioned weaknesses I believe that the set of experiments presented in this chapter contributed to filling in some gaps in the intergroup contact literature with regards to the effectiveness of indirect types of contact in bringing about positive intergroup outcomes, analogous to those of direct contact. By utilizing a rather conservative experimental design I managed to show that indirect forms of contact can yield effects that are comparable to the effects of direct contact. Indirect types of contact were revealed to be particularly useful in emotionally preparing individuals for a future intergroup encounter but were less likely to encourage these individuals to pursue contact. Intergroup anxiety reduction has been found by a number of studies in the past to precede behavioural intentions (e.g., Husnu & Crisp 2010b; Turner et al., 2013). In other words the reduction of intergroup anxiety is a first step towards actively approaching the outgroup and vicarious and indirect types of contact were found to be able to help individuals take this first step.

My contention is that individuals, in order to be ready to pursue contact need, apart from feeling more comfortable with the idea of an intergroup encounter (see intergroup anxiety), to have the confidence or the perceived self-efficacy rather that they can actually go through a face-to-face interaction with an outgroup member successfully. Perceived self-efficacy (see Bandura, 1977) is a construct that has emerged recently in the sphere of indirect contact (see Mazziotta et al., 2010; Stathi et al., 2011), and it is a construct that I believe would have great applications for individuals whose major obstacle in interacting with the outgroup is not their negative evaluations of the outgroup but rather their feelings of inefficacy about sustaining intergroup encounters. The construct of perceived self-efficacy is one I deal with in one of the experiments in the next chapter.

Lastly, in this set of studies the outgroup was always the same while the aim was for the participants to have a positive interaction. In real life though, the outgroup is rarely uniformly positive and thus the valence of the interaction can vary based on who the outgroup is. An idea I chose to investigate in the studies presented next looked into how intergroup contact's effects varied based on who the outgroup was presented to be. I chose to do that in the context of imagined contact where I manipulated how similar, how different, or how similar as well as different the outgroup was from the individual imagining the interaction. I thought of this additional information given on the outgroup as a sort of elaboration of the imagined contact scenario and the consequences of each elaborated scenario were examined.

Chapter 4: Elaborating on the effects of imagined contact: incorporating similarities and differences to the imagined contact scenario

I concluded the previous chapter by remarking that in Experiments 1-3 the emphasis was on exposing the participants to a positive interaction. The pleasantness of the interaction is of paramount importance given how detrimental the effects of negative contact can be (e.g., Barlow, Paolini, Pedersen, Horsney, Radke, Harwood, Rubin, & Sibley, 2012; Paolini, Harwood, & Rubin, 2010). However, no attention was placed on the characteristics of the outgroup member. In reality though, the outgroup member is never uniformly positive and this is something contact interventions rarely take into account. Within intergroup contact research, one of the very few dimensions on which the outgroup member has been differentiated is the dimension of typicality (see e.g., Ensari & Miller, 2002; Hewstone, 1994; Wilder, 1984). Wilder (1984), for example, manipulated the typicality of the outgroup member (as well as the pleasantness of the interaction) to find an overall main effect of typicality: interacting with a typical rather than an atypical outgroup member led to more positive evaluations of the outgroup. Wilder (1984) concluded that interacting with a typical outgroup member is beneficial because it is more predictive of the personality traits as well as the actions of other outgroup members. Typicality was shown to play a role even when individuals were asked to interact with an imaginary outgroup member who was either typical or atypical of his/her outgroup (see Stathi et al., 2011). In the study of Stathi et al. imagining a typical rather than an atypical outgroup member led participants to report more efficacy in interacting with the outgroup.

While the typicality of the outgroup member is a dimension that proved fruitful in the area of contact research, a problem emerges when who is a typical versus an atypical outgroup member is

not straightforward. I presume that in those instances in which the outgroup is alien to the ingroup, the characteristics of a typical outgroup member may sometimes, but certainly not always, be less clear-cut. Greek and Turkish Cypriot youth are an example of such an instance. Individuals of this generation were born and raised in isolation from each other, having no opportunity to even *meet* an individual from the other side of the border before the partial lifting of the travel restrictions.

One could of course argue that inferences on outgroup typicality are not necessarily a product of direct experience, but a product of representations dispersed through the media, education, official and unofficial narratives. In Cyprus' case, however, the most commonly represented 'other' in the aforementioned media, have not been the Turkish Cypriots but the Turks (Spyrou, 2002). While a typical Turk, at least to the minds of Greek Cypriot children, is someone who is 'barbaric', 'quarrelsome', 'wild', and 'dirty' among other things (see Spyrou, 2002), the same rigid representations do not accompany the Turkish Cypriot community. What constitutes a typical Turkish Cypriot in other words is less clear, less certain, and less agreed upon.

Another dimension on which the outgroup members may vary is the outgroup member's similarity to the ingroup member (see e.g., Brewer, 1979; Brown 1984; Roccas & Schwartz, 1993). There is a basic difference between this dimension and the typicality dimension which is that the point of reference for judging the outgroup member in the similarity dimension is not anymore the 'other' or his/her outgroup, but one's own self. The outgroup member can be either similar to oneself or different to oneself, and for one to be able to determine that one does not need to know anything about the outgroup.

The first experiment presented in this chapter utilized the similarity dimension within the paradigm of imagined contact. Participants were asked to imagine a positive interaction with an outgroup member who differed across conditions in terms of his/her similarity with themselves and their ingroup. I more specifically came up with three different imagined contact scenarios based on the similarity dimension. The imagined contact scenarios in all conditions asked participants to imagine engaging in a positive interaction with a member of the outgroup during which they find out that: 1) they (and their ingroup) share a lot of similarities with the outgroup member and his/her group ('high similarity' condition), 2) they (and their ingroup) are different from the outgroup member and the outgroup ('low similarity' condition), and 3) they (and their ingroup) share a number of similarities but also have a number of differences with the outgroup member and the outgroup as a whole ('balanced similarity' condition). The reasoning leading to the three specific conditions will be explained next. The main goal of Experiment 4 was to assess which of the aforementioned imagined contact conditions would lead to the greatest liking of the outgroup.

The imagined contact condition yielding the best intergroup outcomes in Experiment 4 was compared in Experiment 5 with the positive imagined contact condition (see Crisp et al., 2008; 2009) and a no-contact control condition. The three conditions were compared on a set of intergroup-related outcome variables. Since, as I argued in previous chapters, indirect forms of contact (including imagined contact) are best thought to be able to prepare individuals for a future face-to-face interaction by reducing their anxiety and increasing their willingness to approach the outgroup, the two imagined contact conditions were tested against each other (and against the control condition) in terms of their effectiveness in bringing about these effects. Furthermore Experiment 5 incorporated one more criterion variable, namely contact

self-efficacy, i.e., “[the] set of beliefs about one’s ability to interact effectively with outgroupers” (Stathi et al., p. 276) with the aim of testing whether intergroup anxiety and contact self-efficacy do indeed mediate the route from imagined contact to positive action tendencies (as I argued in the discussion of Chapter 3).

EXPERIMENT 4

Is similarity a good thing in intergroup encounters?

Allport (1954) stated that intergroup contact may reduce prejudice given that “...(contact) is of a sort that leads to the perception of *common* interests and *common* humanity between members of the two groups” (p. 281, emphases added). Brown and Lopez (2001) remarked that this statement was perceived as Allport’s support for the importance of intergroup similarity for intergroup contact. This, along with extensive research yielding evidence for the idea that similarity leads to attraction, at least at an interpersonal level (see similarity-attraction hypothesis, e.g., Byrne, 1971; Duck, 1977), led researchers to express the opinion that intergroup interventions should be particularly attentive to promoting similarities between groups (see Stephan, 1999).

The hypothesis that intergroup similarity can be beneficial for intergroup relations has been given support by a number of studies showing that perceived or induced intergroup similarity yields better intergroup relations (e.g., Brewer, 1979; Brewer & Campbell, 1976; Osbeck, Moghaddam, & Perreault, 1997; Roccas & Schwartz, 1993). The same hypothesis has, however, been a source of concerns too, arising predominantly from the field of Social Identity Theory (SIT) (Tajfel & Turner, 1986) whose core tenets brought it in direct opposition to the similarity-attraction theory.

According to SIT, individuals strive to be members of distinct social groups with the aim of boosting or preserving their (high) self-esteem. Self-esteem is preserved or heightened by positively discriminating one's ingroup from relevant outgroups. Based on SIT, Brown (1984a) argued that intergroup similarity can cause a reactionary response from the ingroup who, in their effort to maintain their distinct identity, may strive to differentiate the ingroup from the outgroup. Induced similarity can cause low distinctiveness, which underlies intergroup differentiation (see Jetten, Spears, & Postmes, 2004). This objection to the similarity-attraction hypothesis is known as the similarity-differentiation hypothesis (see Brown & Abrams, 1986; Genthner, Shuntich, & Bunting, 1975; Grant, 1993; Mummendey & Schreiber, 1984) whereby intergroup similarity leads to intergroup differentiation instead of attraction, due to an increase in distinctiveness threat.

The two hypotheses may seem contradictory, but the results of research that has tested similarity-attraction and similarity-differentiation hypotheses in the same study suggest that this is not necessarily the case. These studies have shown that the effectiveness of intergroup similarity in promoting better intergroup relations depends on the context as well as the way in which intergroup similarity is induced. Brown (1984b), for example, found that when intergroup similarity was induced in a cooperative context it led to similarity-attraction, whereas when it was induced in a competitive context it led to intergroup bias. Similarly, Grant (1993) found that when the identity of the ingroup was threatened, the outcome of the intergroup similarity manipulation was ingroup – outgroup differentiation, whereas in a condition in which the ingroup's identity was not threatened the similarity-attraction hypothesis was supported.

Studies have also shown that the degree of similarity induced can have an aversive effect on similarity-attraction. Brown and Abrams (1986) created competitive (the two groups were negatively interdependent) vs. cooperative (the two groups were positively interdependent) contexts and manipulated attitudinal as well as status similarity between the ingroup and the outgroup. They found that when the two groups were presented as similar in attitudes, this led to a more favourable evaluation of the outgroup irrespective of the context, whereas when the two groups were presented as similar both in attitudes *and* in status the outgroup was evaluated less favourably in both contexts. Brown (1988), in interpreting Brown and Abrams' (1986) results, argued that only high levels of similarity give rise to intergroup differentiation. Roccas and Schwartz's (1993) study provided partial support for this claim; they induced varying levels of ingroup-outgroup similarity (moderate, high, and very high) and found that the more similar the two groups were presented as being, the more intergroup bias was reported by the participants.

Summarizing the research to date, intergroup similarity may not lead to attraction and it may, in fact, have the opposite effect (promoting striving for differentiation) under conditions of identity threat (Grant, 1993) and when the context promotes competition rather than cooperation between the two groups (Brown, 1984b). In competitive contexts individuals are more likely to engage in ingroup-favouring evaluations in order to positively distinguish their group. Lastly, even in contexts that are not overtly competitive or threatening to one's identity, very high levels of similarity can also cause intergroup bias. This is because high levels of intergroup similarity may give rise, as predicted by SIT, to distinctiveness threat paired with the need to positively distinguish one's ingroup from the outgroup.

Current study

In Experiment 4 I set out to explore the importance of stressing similarities in an intergroup contact setting by incorporating information about ingroup-outgroup similarities into an imagined contact situation. Given that the intergroup contact context is typically a cooperative and not a competitive setting, it should, in theory, accommodate similarity induction (see Brown, 1984b). However, every instance of intergroup contact between naturally existing groups (e.g., ethnic groups) happens within a greater socio-political context. My argument is that particularly in settings where segregation is prominent and direct contact limited if existent, the intergroup contact setting, even when positive, will not be free of competition. Furthermore, the immediate context on its own, as noted above, is not the only determinant of whether similarity induction will lead to attraction. The other parameter is the amount of similarity induced. As mentioned earlier, high levels of similarity even when induced in cooperative contexts can cause distinctiveness threat which is detrimental to the outcomes of similarity induction as it goes hand in hand with striving for intergroup differentiation. I believe that especially in contexts in which groups have traditionally strived to maintain their distinctiveness (e.g., Catholics and Protestants in Northern Ireland, Greek Cypriots and Turkish Cypriots in Cyprus), high levels of similarity *will* cause distinctiveness threat which will negatively impact one's evaluation of the outgroup.

I, however, thought that there could be a similarity-based manipulation that would not have the aversive effects that other similarity-based manipulations have. As the main danger linked to similarity induction has to do with distinctiveness threat, I sought a manipulation of similarity that would provide a buffer against distinctiveness threat. In doing so I applied the Mutual Intergroup Differentiation Model (Brown & Hewstone, 2005; Hewstone & Brown,

1986), which proposes that for intergroup contact to have its desired positive effects, the identities of the two groups should not be threatened. Brown and Hewstone proposed two ways by which identity threat can be minimized. The one that has received the most attention in contact research (including imagined contact research, see Stathi et al., 2011) is the preservation of group salience during contact. The other way of not threatening group distinctiveness, according to the same authors, is the recognition of each group's areas of expertise as opposed to attempting to eliminate the difference between the two groups. The idea of recognising group differences is central to what Moghaddam and Solliday (1991) called 'balanced multiculturalism'. They argue that it is equally important to strengthen feelings of security and pride, both towards the self or the ingroup (pride in self), as well as towards the other (pride in others or pride in the diversity of the others), so that the differences are celebrated rather than disregarded.

A potential remedy for the distinctiveness problem should therefore involve: a) maintaining the salience of the groups or, in other words, maintaining the intergroup character of the interaction throughout the (imagined) encounter and b) highlighting both the similarities and the differences between the two groups. Such a remedy also satisfies the basic tenets of the Optimal Distinctiveness Model (Brewer, 1991) according to which individuals strive to satisfy two competing needs, the need for assimilation (belonging) and the need for differentiation (uniqueness). In other words individuals do not want to feel either too similar to or too different from others.

I therefore came up with a novel imagined contact scenario in which individuals had to imagine themselves positively interacting with an outgroup member who turned out to be in some ways similar and in other ways dissimilar to them. In order to maintain group salience I also

asked the participants to imagine that through the interaction they also notice similarities and differences between their own and the outgroup's respective ingroups. I termed this new form of similarity-induction, 'balanced similarity', inspired by Moghaddam and Solliday's (1990) use of the term, 'balanced multiculturalism.'

Jetten and Spears (2003) contended that in the same way that similarities can lead to intergroup differentiation, the belief that groups are too different can also produce negative intergroup effects. The idea that intergroup differences can lead to intergroup differentiation is neither new nor counter-intuitive. Even SIT theorists accept the possibility that intergroup differences and not just similarities can become the basis of intergroup bias (see Tajfel & Forgas, 1981). As Jetten and Spears (2003) eloquently put it using Freudian terminology: "...reactions to groups [can be] driven by the "narcissism of small differences" or the "neurosis of large differences"" (p. 205). In addition, the Optimal Distinctiveness Model (Brewer, 1991) would also predict that individuals will be more content in a situation where they find themselves to belong, without their uniqueness being at stake, which means that they would probably react negatively to situations that do not satisfy those needs, i.e., situations in which they either feel too similar or too different to others. To test these contentions, I included one more condition in this experiment in which solely differences between the two group members and their respective groups were emphasised. I will refer to this condition as the 'low similarity' imagined contact condition.

The primary goal of Experiment 4 was to assess the ability of three different imagined contact conditions to lead to more favourable outgroup attitudes. The three imagined contact conditions were: 1) 'high similarity' condition: focusing only on similarities between the ingroup and the outgroup and their respective ingroups, 2) 'balanced similarity' condition:

highlighting both similarities *and* differences of the ingrouper and the outgroup and their respective ingroups, and 3) ‘low similarity’ condition: emphasizing differences between the ingrouper and the outgroup and their ingroups. As discussed above, a presentation of similarities that does not address the threat of distinctiveness could lead to less positive or even negative intergroup outcomes. For this reason I explored whether any differences in the outcomes of the ‘balanced’ and the ‘high’ similarity conditions could be attributed to differences in the distinctiveness threat each condition elicited.

Hypotheses

Hypothesis 1: The ‘balanced similarity’ condition was expected to lead to more favourable outgroup attitudes in comparison to both the ‘low’ and the ‘high’ similarity conditions.

Hypothesis 2: In line with Jetten and Spears (2003) and the Optimal Distinctiveness Model (Brewer, 1991) I anticipated that outgroup attitudes would not differ between the two conditions in which either solely differences or solely similarities were emphasized.

Hypothesis 3: I expected the difference between the ‘high similarity’ and the ‘balanced similarity’ condition on outgroup attitudes to be mediated by reduced distinctiveness threat, whereas I expected this not to be the case for the difference between the ‘balanced similarity’ and the ‘low similarity’ conditions on the same measure, because neither of these conditions should engender threat to distinctiveness.

Method

Participants and design

A total of 60 Greek Cypriot students at the University of Cyprus were randomly allocated to each condition of a one-factor, between-subjects design (20 participants/

condition): ‘high similarity’ condition (13 females, 7 males), ‘low similarity’ condition (14 females, 6 males), ‘balanced similarity’ condition (14 females, 6 males). The participants received a place in a raffle for a prize of 100Euro.

Procedure

Upon coming to the lab the participants were sat in a dimly lit booth. Participants were asked to imagine a scene that would be given to them in the form of a scenario. They were told that they would have one minute to imagine the scene, and that immediately after this one-minute imagery period they would be given a questionnaire to fill in that would include questions on the scene they had imagined. This is why, they were told, they had to try to imagine the scene as vividly as possible. After being given these basic instructions the participants signed the consent form and the experimenter proceeded to present the participants with the scenario corresponding to the condition they were randomly allocated to.

The introduction of each scenario was the same across all conditions; the participants had to imagine that while sitting at a café on a commercial street they happened to meet a young Turkish Cypriot of about the same age and the same gender as themselves, and that they strike up a conversation with her or him. The participants were then asked to imagine that through the conversation, they discovered that the Turkish Cypriot stranger was in fact:

- a) Very similar to them and their ingroup in the 'high similarity' condition:

“[...] During the conversation you find out that you and Ayshe/ Dervish have lots in common. You find out that the hopes as well as the worries young Turkish Cypriots have for the future (e.g., studies, employment, living up to your parents’

expectations) are similar to the ones that you and many other young Greek Cypriots have. After the conversation ends you start to think that you have a lot of similarities with young Turkish Cypriots”; or

- b) Different from them and their ingroup (‘low similarity’):

“ [...] During the conversation you find out that you are very different from Ayshe/ Dervish and other young Turkish Cypriots. It turns out that Ayshe/ Dervish as well as young Turkish Cypriots have a very different way of viewing the future and life in general in comparison to you and most other young Greek Cypriots. After the conversation ends you start to think that you are quite different from young Turkish Cypriots”; or

- c) Similar to as well as different from them and their ingroup (‘balanced similarity’):

“ [...] During the conversation you find out that you and Ayshe/ Dervish are in some ways very similar to, and in other ways very different from, each other. You find out that the same thing goes for young Greek and Turkish Cypriots, i.e., that they are both different and similar especially with regards to how they view their studies, their career, and their effort to live up to their parents’ expectations. After the conversation ends you start to think that you and young Turkish Cypriots are in some ways very similar and in some ways very different to each other”.

They were also told that in order to facilitate the mental imagery task, the experimenter would turn off the lights thus leaving the room very dark. The experiment went through the scenario once together with the participants before leaving them for a minute to go through the

scenario themselves. Then the experimenter came back to the room to let the participant know that the imagery period was about to start. The experimenter before leaving the room reminded the participants that they had to try to imagine the scene as vividly as possible. When the time (one minute) was up, the experimenter re-entered the room, and asked the participant to complete a questionnaire. A question preceded the actual questionnaire, requesting from the participants to describe in as much detail as they wished the scene they had imagined. This was done for purposes of strengthening the effect of the manipulation (Crisp et al., 2009).

Measures

Manipulation-check items

In order to test whether the similarity induction was successful, two different measures were used. For both measures, participants had to respond on a 7-point scale (1, *extremely dissimilar*; 7, *extremely similar*). The first measure tapped on perceived similarity between the self and the imagined individual (*perceived interpersonal similarity*) and it was measured using one item: 'How similar or dissimilar do you see yourself to be in comparison to the individual you imagined interacting with?' The second measure addressed perceived similarity between ingroup and outgroup (*perceived intergroup similarity*) and it was comprised of six items (Cronbach's $\alpha = .88$). The participants were asked to rate to what extent they thought that Turkish Cypriots are similar to Greek Cypriots in terms of: 'the way they see things', 'their habits', 'their values', 'their food habits', 'their love for Cyprus', 'the things they like to do'.

Dependent Variable

Attitudes towards Turkish Cypriots were measured by an 11-point thermometer-like scale (Converse & Presser, 1986) on which participants had to indicate how they felt towards Turkish Cypriots. The lower point (1 = 0 degrees) stood for extremely unfavourable attitudes and the upper point (11 = 100 degrees) represented extremely favourable attitudes. The midpoint (6 = 50 degrees) indicated neither favourable nor unfavourable attitudes.

Mediator

Distinctiveness threat was measured via two items (Cronbach's $\alpha = .77$) that were rated on a 7-point scale (1, *strongly disagree*; 7, *strongly agree*). The items were: 'The two main communities in Cyprus should simply be seen as Cypriots' (reverse coded), 'It annoys me when others see Greek Cypriots and Turkish Cypriots as the same thing'.

Control variable

Prior outgroup friendships were measured with one item asking the participants how many Turkish Cypriot friends they have (1, *none*; 5, *a lot*).

Results

Table 4.1 shows the means and standard deviations of each condition on all measures and *Table 4.2* presents the inter-correlations between all the variables. I first compared the three conditions on the manipulation-check measures, while controlling for distinctiveness threat, to assess the success of the manipulation. Secondly, I compared the three conditions on outgroup attitudes in order to test Hypotheses 1 and 2, and then proceeded with mediation tests to check whether any differences emerging between the 'balanced similarity' condition and the 'high similarity' condition were at least partly due to the fact that the two conditions

elicited different levels of distinctiveness threat (Hypothesis 3). There were no systematic effects of gender, so I collapsed over that factor in all reported analyses. Furthermore, I did not have to control for prior outgroup friendships as there was no significant relationship between that variable and the dependent variable (outgroup attitudes) and because the three groups did not differ along this variable ($F(2, 57) = .21, p = .81, \text{partial } \eta^2 = .01$).

Table 4.1. Means and standard deviations on all variables for the three conditions

	High Similarity (N=20) <i>M (SD)</i>	Low Similarity (N=20) <i>M (SD)</i>	Balanced Similarity (N=20) <i>M (SD)</i>
Interpersonal similarity	4.85 (1.42)	3.95 (1.61)	5.05 (1.39)
Intergroup similarity	4.03 (1.18)	3.38 (1.21)	4.30 (0.99)
Outgroup attitudes	5.65 (2.60)	5.75 (2.40)	7.30 (1.81)
Distinctiveness threat	4.15 (1.24)	3.96 (1.63)	3.60 (1.49)
Outgroup friends	1.10 (0.45)	1.15 (0.49)	1.20 (0.52)

Table 4.2. Inter-correlations between all variables

	1	2	3	4	5
Interpersonal Similarity	--	.55**	.51**	-.63**	.17
Intergroup similarity		--	.65**	-.66**	.30*
Outgroup attitudes			--	-.66**	.21
Distinctiveness threat				--	-.10
Outgroup friends					--

* $p < .05$, ** $p < .01$

Manipulation check

In order to assess whether the similarity manipulation was successful, a Multivariate Analysis of Covariance (MANCOVA) was employed with the two manipulation measures as the dependent variables, condition as the independent variable, and distinctiveness threat as a covariate. I included distinctiveness threat as a covariate because I expected it to influence the way participants perceived the outgroup and the outgroup as a whole⁵⁴. The MANCOVA using Pillai's trace yielded a significant multivariate effect for condition ($V = .25$, $F(4, 112) = 4.06$, $p < .01$, partial $\eta^2 = .14$). A look at the univariate effects showed a significant effect of condition on both perceived interpersonal similarity ($F(2, 56) = 4.81$, $p < .05$, partial $\eta^2 = .15$) and perceived intergroup similarity, ($F(2, 56) = 5.36$, $p < .01$, partial $\eta^2 = .16$).

I expected that the two conditions including similarities ('high similarity' and 'balanced similarity' condition) would be differentiated on these two manipulation-check variables in comparison to the condition that included no similarities ('low similarity' condition). For this purpose simple contrasts were conducted to assess whether each of the first two conditions differed from the 'low similarity' condition along these two variables. Simple contrasts⁵⁵ for the variable perceived interpersonal similarity yielded significant differences both between the 'high similarity' and the 'low similarity' condition ($p = .006$) as well as between the 'balanced similarity' and the 'low similarity' condition ($p = .02$), with the two conditions in which similarity was induced to lead to higher perceived interpersonal

⁵⁴ According to Field (2009), including covariates may be useful in two ways: a) it reduces the within-group error variance (by explaining some of the 'unexplained' variance via other variables, i.e., the covariates), which leads to a more accurate estimation of the effect of the independent variable, and b) it eliminates the variables other than the experimental variables that affect the outcome variable.

⁵⁵ A more conservative p -value was adopted for the simple contrasts, as these contrasts are not orthogonal. The new p -value, corrected for multiple comparisons, was $p = .025$.

similarity. The findings for the variable perceived intergroup similarity were similar for which both the ‘high similarity’ and the ‘balanced similarity’ conditions led to higher scores in comparison to the ‘low similarity’ condition ($p = .006$; $p = .008$ respectively). Furthermore, linear polynomial contrasts assessed whether for each of the two manipulation-check variables the order in which the conditions ranked was the expected one. Namely, the ‘high similarity’ condition was expected to lead to the highest perceived interpersonal and intergroup similarity followed by the ‘balanced similarity’ condition and then by the ‘low similarity’ condition. Both polynomial contrasts were significant (interpersonal similarity: $p = .006$; intergroup similarity: $p = .006$) thus showing that the manipulation of the similarity was successful.

Next, I assessed whether the manipulation produced differences on the dependent variable (i.e., attitudes towards Turkish Cypriots). For this purpose I conducted a univariate Analysis of Variance (ANOVA) to determine whether there were significant differences on the dependent variable across conditions. The analysis yielded a significant overall effect of condition on attitudes, $F(2, 58) = 3.25$, $p < 0.05$, partial $\eta^2 = .10$ ($M_{\text{balanced similarity}} = 7.30$, $M_{\text{high similarity}} = 5.65$, $M_{\text{low similarity}} = 5.75$). In order to test Hypothesis 1 and whether the ‘balanced similarity’ condition led to more favourable attitudes in comparison to each of the other two conditions, I used simple contrasts. These contrasts yielded marginal⁵⁶ differences between ‘balanced similarity’ and ‘high similarity’ ($p = .027$) and between ‘balanced similarity’ and ‘low similarity’ ($p = .037$) conditions and hence largely supported Hypothesis 1.

To test Hypothesis 2, Helmert contrasts were used. For this purpose I ordered the three conditions as follows: 1) ‘balanced similarity’, 2) ‘high similarity’, 3) ‘low similarity’. The two contrasts computed were: Contrast (A): 2, -1, -1 (which tested whether the ‘balanced

⁵⁶ Since simple contrasts are non-orthogonal I used a more conservative p -value ($p = 0.025$)

similarity' imagined contact condition yielded more positive outgroup attitudes compared with the other two conditions combined); and Contrast (B): 0, 1, -1 (tested whether there was a difference between the 'high' and the 'low' similarity conditions on the dependent variable). Contrast A was significant ($p < 0.05$), indicating that the 'balanced similarity' imagined contact condition promoted more favourable attitudes towards the outgroup compared with the other two conditions combined. Contrast B, in line with Hypothesis 2, was not significant, ($p = 0.89$) indicating that there was no difference between the 'low similarity' and the 'high similarity' conditions in terms of the dependent variable.

Since the 'balanced' and 'high' similarity conditions differed on the outcome measure, I proceeded to test Hypothesis 3 according to which the differential effects of the two conditions on outgroup attitudes would be attributed to distinctiveness threat. For this purpose, I conducted a mediation analysis using bootstrapping⁵⁷ in order to check the significance of the indirect effect of condition ('balanced similarity' (+1) vs. 'high similarity' (-1)) on outgroup attitudes via distinctiveness threat. The indirect effect of condition on attitudes towards Turkish Cypriots was significant at the 0.05 level (point estimate: 0.49 with a 95% confidence interval of [0.02, 1.71]). I conducted the same analysis in order to test whether distinctiveness threat mediated the effect of 'balanced' vs. 'low similarity' conditions on the outcome variable, and found no mediation effect.

Discussion

The results of Experiment 4 confirmed my hypotheses. The 'balanced similarity' condition yielded the most positive outgroup attitudes and it was significantly different from

⁵⁷ Bias Corrected Bootstrapping techniques are favoured over conventional forms of mediation tests (e.g., Sobel's Z) because of their superior ability to detect significant mediation effects with smaller sample sizes (when compared to other techniques) while retaining the most power (see Fritz & MacKinnon, 2007).

the other two conditions combined, as well as from each of them separately (albeit marginally so). There was no significant difference between the ‘low’ and the ‘high’ similarity imagined contact conditions. This finding is in agreement with both the Optimal Distinctiveness Model (Brewer, 1991), according to which individuals will prefer to be in a situation that satisfies their needs of uniqueness (differentiation) and belongingness (assimilation), and with Jetten and Spears’ (2003) contention that both small and big differences can cause negative intergroup outcomes (e.g., less favourable attitudes as in this experiment).

The results are also in agreement with the Mutual Intergroup Differentiation Model of Hewstone and Brown (1986; Brown & Hewstone, 2005). Intergroup contact, in this case imagined contact, benefited from the protection of identity distinctiveness which was achieved through a simultaneous highlighting of both similarities and differences between the two groups while also preserving group salience. It should be noted here that group salience was maintained in all conditions (by asking participants to think of similarities, or differences, or similarities/ and differences not just between themselves and the imagined individual but between their ingroup and outgroup as well). This suggests that any differences detected across conditions are more likely to be due to the similarity manipulation and not due to salience, as group salience was kept constant across conditions. Measurement of group salience following presentation of the imagery, would have allowed me to corroborate this point. As I did not include this measure I can only speculate that since the intergroup component was stressed in all conditions this led to the participants experiencing some group salience in all conditions but no differences in salience across conditions.

No control condition was used in Experiment 4, and for this reason I was unable to infer from the results whether the three conditions on average yielded positive or negative

attitudes. Consequently, even though it is clear from the results that the ‘balanced similarity’ condition led to more favourable outgroup attitudes in comparison to the other two conditions, I could not say with certainty whether this meant that the ‘balanced similarity’ imagined contact condition led to even more positive attitudes (assuming that all conditions yielded positive attitudes) or whether it simply led to less negative attitudes (assuming that all conditions yielded negative attitudes). I therefore included a control condition in the second experiment that served as a point of comparison for the effects of ‘balanced similarity’ on outgroup attitudes.

Furthermore, if the ‘balanced similarity’ imagined contact condition were to be used as an imagined contact scenario for intervention purposes, another key question is how this condition fares as an alternative to the positive imagined contact. The comparison of the two imagined contact conditions and a control condition on a number of theoretically relevant criterion variables was the second goal of Experiment 5.

EXPERIMENT 5

Crisp et al. (2009) claimed that imagining a positive interaction “fulfils the necessary and sufficient conditions to observe positive outcomes” (p. 5). Despite this claim, researchers endeavoured to find ways to enhance the effectiveness of imagined contact, mainly through attempting various amendments to the instructions of the positive contact scenario regarding: a) *how* to imagine the contact situation (i.e., elaboration, see Husnu & Crisp, 2010a, b), b) whether contact is ‘interpersonal’ or ‘intergroup’ (e.g., Pagotto et al., 2012), c) whether the imagined individual is a typical or atypical member of the outgroup (see Stathi et al., 2011; Experiment 3), and d) whether contact incorporates cooperation or not (Kuchenbrandt et al.,

2013). Of these attempts to amend the positive imagined contact scenario, only Kuchenbrandt et al. (2013) challenged the assumption that imagining a positive intergroup interaction is necessary *and* sufficient. They found in their study that the imagined contact scenario that was both positive and cooperative led to more profound changes in comparison to the positive (but not cooperative) contact condition. Crucially, the positive imagined contact condition in Kuchenbrandt et al.'s study, yielded no differences in comparison to a neutral intergroup contact condition on the measures of intergroup anxiety and outgroup trust.

In Experiment 5, I compared the 'balanced similarity' imagined contact condition with the positive imagined contact condition in an attempt to assess whether the proposed scenario (i.e., 'balanced similarity' imagined contact) could add to the already positive effects of the positive imagined contact scenario. I compared the two imagined contact conditions on three more dependent variables other than outgroup attitudes. These were: intergroup anxiety, contact self-efficacy, and action tendencies. As revealed by Experiment 3, the most profound contribution of imagined contact was in reducing the individuals' anxiety for future intergroup interactions. The results of the same experiment yielded weak but traceable support for imagined contact's capacity to promote more positive action tendencies. I argued then that what the individuals might be missing in order to find the courage needed to engage in intergroup contact apart from feeling more comfortable with the idea of an intergroup encounter (i.e., through lowered intergroup anxiety), is the confidence or the perceived self-efficacy that they can sustain a face-to-face interaction with the outgroup. This is what led me to include contact self-efficacy as a criterion variable in this second experiment.

Imagining an interaction which reveals both the similarities and differences between two individuals and their respective outgroups, in my opinion, equips individuals with not just

a positive, but also an informative mental script for future encounters. I argue that if the ultimate goal of imagined contact is to prepare people for actual contact, then the imagined scenario should be an accurate reflection of reality. My contention therefore is that the ‘balanced similarity’ imagined contact condition can have even better effects in comparison to the positive imagined contact condition in preparing individuals for a real interaction with the outgroup because it would provide the individuals with a more rich and realistic imaginary intergroup experience that will push them to see beyond the pleasantness of the interaction and to hopefully venture into actual and potentially sensitive intergroup encounters.

Intergroup anxiety and contact self-efficacy as mediators

In Experiment 3, I did not test intergroup anxiety as a mediator of the effects of (imagined) contact on action tendencies. Intergroup anxiety, however, has emerged as a consistent mediator explaining the effects of (imagined) intergroup contact on behavioural intentions (see e.g., Husnu & Crisp 2010b; Turner et al., 2013). Furthermore, studies on the determinants of behaviour more generally have shown that reported self-efficacy about the behaviour in question is one of the most important determinants of the actual behaviour (see e.g., Ajzen, 1991; Bandura, 1997; Terry & O’Leary, 1995). Within the intergroup contact sphere, Mazziotta et al. (2011) provided support for the mediating role of perceived contact self-efficacy in promoting more positive behavioural intentions. Mazziotta et al. (2011) demonstrated this in the context of vicarious contact. It is yet to be investigated whether imagined contact’s effects on action tendencies can be mediated by contact self-efficacy. In Experiment 5 therefore, I tested both intergroup anxiety as well as contact self-efficacy as mediators of the effect of imagined contact on action tendencies.

Control condition

Deciding on a control condition that would serve its purpose well for both kinds of imagined contact was not simple. As my aim was to compare the 'balanced similarity' imagined contact condition with an effective form of imagined contact, I thought that the priority was to use a control condition that matched the positive imagined contact best so that I establish its effectiveness in my context of interest. I therefore chose the most commonly used control condition in imagined contact experiments which is the imagination of a positive scene (see e.g., Turner & Crisp, 2010; Turner et al., 2007c; and see Crisp & Turner, 2009; 2012, for a discussion of the merits of various possible control conditions).

In summary, the three main goals of Experiment 5 were: 1) to investigate whether imagined contact (both the 'balanced similarity' and the positive imagined contact conditions) had an effect on outgroup attitudes, intergroup anxiety, and self-efficacy in comparison to the control condition, 2) to assess whether these effects were more profound for the 'balanced similarity' condition as opposed to the positive imagined contact condition, 3) to test whether imagined contact had an indirect effect on positive action tendencies via intergroup anxiety and contact self-efficacy. Since prior outgroup friendships were found to be non-existent in the first experiment (Experiment 4), I replaced the prior outgroup friendships measure with a more general intergroup contact measure in Experiment 5 (see *Measures* section for more).

Hypotheses

Hypothesis 1: I expected both imagined contact conditions to be significantly different from the control condition on outgroup attitudes, intergroup anxiety, contact self-efficacy, and on action tendencies.

Hypothesis 2: I predicted that the two imagined contact conditions would differ from each other particularly on the two variables that are most related to preparing one for future contact, i.e., intergroup anxiety and contact self-efficacy. I expected the ‘balanced similarity’ condition to yield (even) less intergroup anxiety and (even) higher contact self-efficacy.

Hypothesis 3: I expected that imagined contact would have an indirect effect on action tendencies via intergroup anxiety and contact self-efficacy.

Method

Participants and design

Fifty Greek Cypriot undergraduate students (all female) studying at the University of Cyprus were randomly allocated to one of three conditions in a between-subjects design: a) ‘balanced similarity’ imagined contact condition (N=17); b) positive imagined contact condition (N=17), and c) control condition (N=16). All participants were entered in a raffle for a prize of 100 Euros.

Procedure

The procedure was the same as in Experiment 4 with the only difference being the content of the scenarios the participants had to imagine. I used the positive-outdoor-scene control condition (see Turner et al., 2007). The instructions were:

“I will now ask you to close your eyes and imagine that you are in an outdoor place in Cyprus that you like a lot. The day is beautiful and you feel good. Try to imagine the scene as vividly as possible”.

The instructions for both imagined contact scenarios entailed the elements of elaboration (see Husnu & Crisp, 2010) and high group salience (see Stathi et al., 2011; Expt. 1) so that the positive imagined contact condition's scenario was as compatible as possible with the 'balanced similarity' imagined contact condition's scenario which incorporated both of these elements (i.e., specifying the time and the place of the interaction and maintaining the intergroup element of the interaction). The instructions therefore for the scenario of the positive imagined contact condition were as follows:

“I will now ask you to close your eyes and imagine that you meet a Turkish Cypriot girl for the first time. Think specifically where the meeting takes place and when. From your discussion you find out various interesting things both about the Turkish Cypriot girl as well as about young Turkish Cypriots as a whole”.

Lastly, the instructions for the scenario of the 'balanced similarity' imagined contact condition were as follows:

“I will now ask you to close your eyes and imagine that you meet a Turkish Cypriot girl for the first time. Think specifically where the meeting takes place and when. During the meeting you find out that you and the Turkish Cypriot girl are in some ways similar and in other ways different from each other. This is also what you find out about yourself and young Turkish Cypriots in general, i.e., that you are in some ways similar to and in other ways different from them”.

Measures

Dependent variables

Attitudes towards Turkish Cypriots were measured in the same way as in Experiment 4, i.e., via a feeling thermometer (1, 0 degrees; 11, 100 degrees).

Intergroup anxiety was measured using a 4-item measure (Cronbach's alpha = .73). Participants were asked to answer how “comfortable” (reverse coded), “safe” (reverse coded), “suspicious”, and “threatened”, they would feel at the prospect of meeting and having a talk with a Turkish Cypriot (1, *not at all*; 7, *extremely*).

Contact self-efficacy was measured with three items (Cronbach's alpha = .69) that were adapted from Fan and Mak's (1998) measure of social self-efficacy. The items were: “I think it will be easy for me to talk to Turkish Cypriots”, “I am sure that I will get on well with Turkish Cypriots I meet”, and “I feel I know enough about Turkish Cypriots to be able to have a good conversation with a member of this community”. All items were rated on a 7-point scale (1, *strongly disagree*; 7, *strongly agree*).

Action tendencies were measured via a 3-item scale (Cronbach's alpha = .80). The instructions asked participants how willing they would be to: “*have a Turkish Cypriot as a friend*”, “*accept a Turkish Cypriot as a family member*”, “*know more about Turkish Cypriots*” (1, *not at all*; 7, *extremely*).

Control variable

Prior intergroup contact was measured via one item asking participants how much contact they had had with Turkish Cypriots (1, *no contact at all*; 5, *a lot of contact*).

Results

Table 4.3. shows the means and standard deviations for all measures in each condition and *Table 4.4.* outlines all inter-correlations between the variables. Univariate ANOVAs were conducted to determine whether there was a main effect of condition on the criterion variables: outgroup attitudes, intergroup anxiety, contact self-efficacy, and action tendencies. Since prior intergroup contact did not correlate with any of the criterion variables and given that the three conditions were not different on this variable ($F(2, 47) = 1.19, p = .31$, partial $\eta^2 = .05$) it was left out of the analyses.

The results of each univariate ANOVA are presented next for each of the dependent variables. In the case where a significant effect of condition existed I looked into simple contrasts to determine whether each of the imagined contact conditions differed from the control condition (Hypothesis 1). For all simple contrasts a corrected-for-multiple-comparisons p -value was adopted ($p = .025$). I then looked into the Helmert contrasts to determine whether the two imagined contact conditions (combined) were different from the control condition and whether there was a difference between the two imagined contact conditions (Hypothesis 2). For these contrasts, I ordered the three conditions as follows: 1) control condition, 2) 'balanced similarity' condition, 3) positive imagined contact condition. The two contrasts computed were: Contrast (A): 2, -1, -1 (which tested whether the two imagined contact conditions combined differed from the control condition); and Contrast (B): 0, 1, -1 (tested whether there were differences between the two imagined contact conditions).

Table 4.3. Means and standard deviations on all variables for the three conditions

	‘Balanced Similarity’ Imagined Contact (N=17) M (SD)	Positive Imagined Contact (N=17) M (SD)	Control Condition (N=16) M (SD)
Outgroup attitudes	7.00 (1.80)	6.59 (1.23)	5.25 (1.57)
Intergroup anxiety	3.53 (1.21)	4.38 (1.00)	4.20 (1.18)
Contact self-efficacy	4.75 (0.87)	4.12 (0.90)	4.06 (0.86)
Action tendencies	4.35 (1.37)	3.71 (1.48)	3.81 (1.32)
Prior contact	2.06 (0.83)	1.94 (0.66)	1.69 (0.60)

Table 4.4. Inter-correlations between all variables

	1	2	3	4	5
Outgroup attitudes	--	-.32*	.59**	.55**	.09
Intergroup anxiety		--	-.51**	-.52**	.04
Contact self-efficacy			--	.20**	.04
Action tendencies				--	.05
Prior contact					--

* $p < .05$, ** $p < .01$

Outgroup attitudes

There was a significant main effect of condition on outgroup attitudes, $F(2, 47) = 5.69$, $p < .01$, partial $\eta^2 = .20$. Simple contrasts, in line with Hypothesis 1 revealed more positive outgroup attitudes in the ‘balanced similarity’ condition than in the control condition

($p = 0.02$), and in the positive imagined contact condition compared with the control condition ($p = 0.017$) ($M_{\text{balanced similarity}} = 6.00$, $M_{\text{positive imagined contact}} = 5.59$, $M_{\text{control}} = 4.25$). Contrast A of the Helmert contrasts was significant ($p < .01$) indicating that the two imagined contact conditions combined yielded different (more positive) attitudes than the control condition. Contrast B was not significant ($p = .44$), hence there was no significant difference between the two imagined contact conditions on this variable.

Intergroup anxiety

The effect of condition on intergroup anxiety was marginal ($F(2, 47) = 2.66$, $p = .08$, partial $\eta^2 = .10$). A look at the simple contrasts indicated, unlike what was hypothesized (see Hypothesis 1), only a weak trend for a difference between the ‘balanced similarity’ and the control condition ($p = .09$) with the ‘balanced similarity’ condition yielding less intergroup anxiety, but no difference between the positive imagined contact and the control condition ($p = .65$) ($M_{\text{positive similarity}} = 3.53$, $M_{\text{standard imagined contact}} = 4.38$, $M_{\text{control}} = 4.20$). Contrast A of the Helmert contrasts comparing the combined effects of the two imagined contact conditions with the control condition was not significant ($p = .48$), whereas Contrast B through which I compared the two imagined contact conditions with each other was significant ($p < .05$). In line with Hypothesis 2, ‘balanced similarity’ imagined contact was found to yield significantly less intergroup anxiety than positive imagined contact.

Contact self-efficacy

There was a significant effect of condition on contact self-efficacy, $F(2, 47) = 3.12$, $p = .05$, partial $\eta^2 = .12$. Simple contrasts, partially in line with Hypothesis 1, yielded a marginally higher contact self-efficacy in the ‘balanced similarity’ condition in comparison to

the control condition ($p = .03$), but no difference between the positive imagined contact condition and the control condition ($p = .86$) ($M_{\text{balanced similarity}} = 4.75$, $M_{\text{positive imagined contact}} = 4.12$, $M_{\text{control}} = 4.06$). The first Helmert contrast (Contrast A) was not significant ($p = .17$) thus indicating that the two imagined contact conditions combined were not different from the control condition on the contact self-efficacy variable. Contrast B, on the other hand, was significant ($p < .05$) thus showing that the ‘balanced similarity’ imagined contact condition gave rise to significantly higher contact self-efficacy than the positive imagined contact condition. This finding yielded support for Hypothesis 2.

Action tendencies

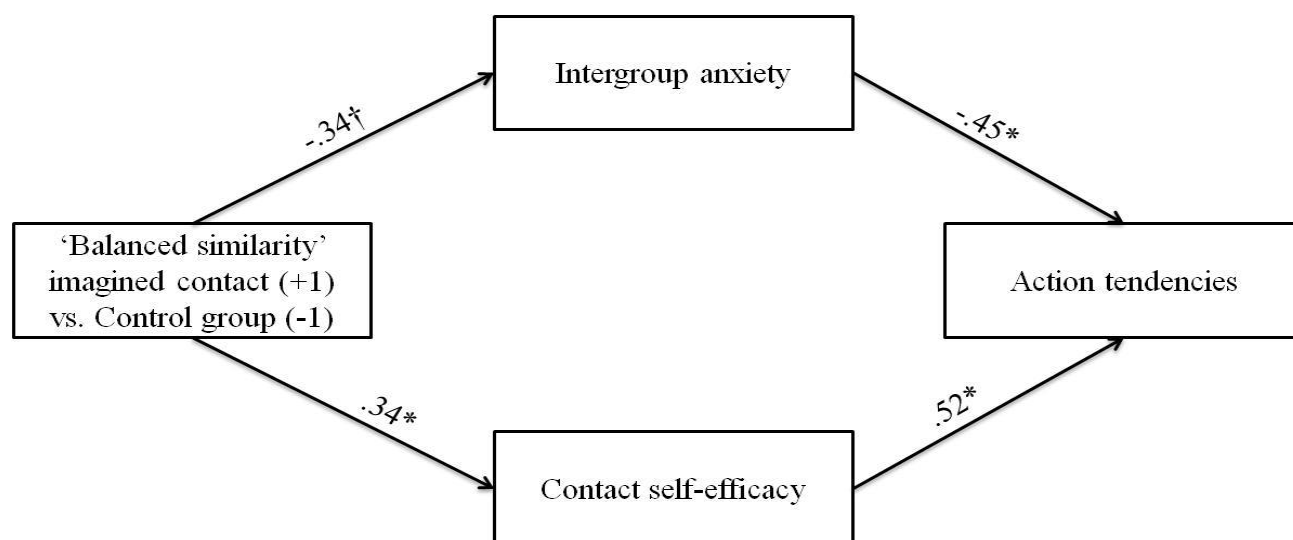
There was no effect of condition on the measure of action tendencies, $F(2, 47) = 1.05$, $p = .36$, partial $\eta^2 = .04$ ($M_{\text{balanced similarity}} = 4.35$, $M_{\text{positive imagined contact}} = 3.71$, $M_{\text{control}} = 3.81$). Despite that, I proceeded to check for an indirect effect of contact on action tendencies via intergroup anxiety and contact self-efficacy (see Hypothesis 3).

Indirect effect of imagined contact on action tendencies

Since only the ‘balanced similarity’ condition was found to differ on the two proposed mediators (albeit only marginally for intergroup anxiety) from the control condition, I tested the indirect effect of imagined contact on action tendencies by including in the mediational analyses the ‘balanced similarity’ (+1) and the control condition (-1). I assessed whether the indirect effect of ‘balanced similarity’ on action tendencies was simultaneously mediated by intergroup anxiety and contact self-efficacy. The mediational analysis using bootstrapping yielded a significant (at the 0.05 level) overall indirect effect of Condition on action tendencies (point estimate: .33 with a 95% confidence interval of [.04, .71]). Of the specific

indirect effects, only the effect of contact self-efficacy was significant at the 0.05 level (point estimate: .18 with a 95% confidence interval of [.01, .44]) whereas the indirect effect of Condition on action tendencies via intergroup anxiety only approached significance (Sobel test analysis: $z = 1.50$, $p_{\text{(one-tailed)}} = .07$, mediational analysis using bootstrapping: point estimate: .15, with a 95% confidence interval of [-.007, .49]). *Figure 4.1.*, presents all paths and their coefficients as produced via the mediational analysis using bootstrapping.

Figure 4.1. Diagram presenting the paths between condition, proposed mediators (intergroup anxiety and contact self-efficacy), and action tendencies



†, $p = .10$, * $p < .05$

Discussion

The results of Experiment 5 confirm the hypothesis that a careful induction of similarities in an imagined contact situation can lead to positive intergroup outcomes that are in fact superior to the outcomes of the well-supported positive imagined contact at least with regards to the measures related to the individual's readiness to pursue and sustain future face-to-face intergroup encounters. The 'balanced similarity' imagined contact yielded, just like the

positive imagined contact, significantly more positive outgroup attitudes in comparison to the control condition but also significantly higher contact self-efficacy in comparison to both the control condition as well as the positive imagined contact condition. Furthermore the ‘balanced similarity’ imagined contact condition was found to have a marginally significant effect on intergroup anxiety reduction in comparison to the control condition as well as significantly less intergroup anxiety in comparison to the positive imagined contact condition.

Even though the ‘balanced similarity’ condition did not yield significantly more positive action tendencies in comparison to the control condition, results showed a significant indirect effect of the ‘balanced similarity’ condition (vs. the control condition) on action tendencies via the variables intergroup anxiety and contact self-efficacy. This is an important finding as it supports the claim that imagined contact is particularly well-suited to prepare individuals for future intergroup encounters and to render them more willing to pursue (further) intergroup contact (see Crisp et al., 2010; Crisp & Turner, 2013). In addition to that, this is the first study testing contact self-efficacy as a mediator of imagined contact’s effects on action tendencies.

The unexpected results found in this experiment pertain to the positive imagined contact condition’s null effects on all variables related to preparing participants for future intergroup contact. Apart from a significant effect on outgroup attitudes, the positive imagined contact condition did not yield any effects on intergroup anxiety or contact self-efficacy in comparison to the control group. These null effects are particularly puzzling, since in Experiment 3 (see Chapter 3) the same imagined contact scenario led to a significant decrease in intergroup anxiety from pretest to posttest, resulting in lower intergroup anxiety in comparison to the control condition at posttest. It should also be noted that while the positive

imagined contact condition had no effects on intergroup anxiety in this study, it did yield more positive intergroup attitudes in comparison to the control condition whereas in Experiment 3 no effects of the positive imagined contact condition were noted for outgroup attitudes.

The interpretation of these contrasting results becomes even more difficult if I take into account that the two samples were entirely similar in terms of their characteristics (i.e., female undergraduate students in universities of Nicosia with minimal prior intergroup contact). What could potentially explain the differential findings of the positive imagined contact condition in the two experiments are some methodological differences they two experiments had. The experimental design in Experiment 3 employed a pretest-intervention-posttest (and delayed posttest) design as well as a control condition. The current study included a control condition but no pretest. As noted in the discussion of the previous chapter, a pretest is, arguably, likely to make the participants more prone to the intervention and maybe this is the reason that the positive imagined contact condition yielded differences in intergroup anxiety in Experiment 3. This explanation, however, cannot account for the different findings in terms of the outgroup attitudes variable. If the pretest did render participants more prone to the intervention, then effects of imagined contact on outgroup attitudes should have been observed in Experiment 3 and not in the current experiment.

Another difference between the two experiments was a slight twist in the scenario of positive imagined contact performed for the purposes of the current experiment. In an attempt to make the positive imagined contact scenario as compatible as possible to the ‘balanced similarity’ imagined contact scenario I added a phrase asking the participants to “find out various interesting things *both* for the Turkish Cypriot girl *as well as* for young Turkish Cypriots as a whole” (emphases added). In Experiment 3 participants were not asked to

imagine finding out interesting things about “the outgroup as whole”. This additional phrase asking the participants to pay attention to both the outgroup individual as well as the outgroup might have resulted in greater group salience in the current study as opposed to Experiment 3. Higher group salience can have both beneficial and harmful effects. On the one hand, higher group salience can assist the generalisation of the attitudes from the outgroup individual to the outgroup as a whole and this can account for the positive effects of the positive imagined contact condition on outgroup attitudes in the current experiment. On the other hand higher group salience can also cause feelings of (intergroup) anxiety (see e.g., Pettigrew, 1998) and perhaps this could explain why the positive imagined contact condition did not lead to an intergroup anxiety reduction in this study.

As far as contact self-efficacy is concerned, according to Bandura (1977), a very influential source of self-efficacy expectations are the individuals’ prior experiences. My argument is that given the practically non-existent prior intergroup experiences of the participants in this study, an increase in contact self-efficacy formed a great challenge for imagined contact. Perhaps precisely because of the total lack of intergroup experience, this set of individuals benefitted from a condition (‘balanced similarity’) that required them to more deeply explore their relations to the outgroup and in this way prepared them for an actual interaction with it.

The null-effect of the positive imagined contact condition on this variable might also suggest that imagining a positive intergroup interaction may be necessary but not sufficient in certain contexts (e.g., a context with low prior intergroup contact as Cyprus is). This study says little as to *why* the two imagined contact conditions produced different results. I think that a useful variable that was missing from this experiment but could have helped to shed

light to the differences or rather the explanation of the differences between the two imagined contact conditions in terms of their effects on contact self-efficacy is the variable of perceived knowledge about the outgroup (see e.g., Eller & Abrams, 2004; Pettigrew, 1997). Future research interested in replicating the effects of the ‘balanced similarity’ condition proposed here can take steps to better investigating the explanatory mechanisms behind the differential effects (if any) of this condition and the merely positive imagined contact condition.

It could also be interesting to know what the effects of the other two conditions (i.e., the ‘low similarity’ and the ‘high similarity’ condition) would be on the measures of intergroup anxiety and contact self-efficacy. My assumption is that even though ‘high similarity’ was found to backfire in terms of how the participants evaluated the outgroup it could have been beneficial in terms of feelings of intergroup anxiety, unlike the ‘low similarity’ condition that I would expect to yield either no differences in intergroup anxiety or even more anxiety. On the contrary, I would not be surprised if the ‘low similarity’ condition emphasizing the differences between the ingrouper and the outgroup and their respective groups could, unlike the ‘high similarity’ condition, lead to more reported efficacy for contact. If individuals manage to imagine having an otherwise positive interaction with an outgroup who turns out to be different from them, I gather that this will make them feel more confident that they can face and successfully go through other intergroup interactions.

General Discussion

The results of the two experiments provide clear evidence that theoretically-based augmentation of standard imagined contact instructions can further increase the effectiveness of this promising intervention to improve intergroup relations. In this section I will summarize

the main findings of the two experiments before I discuss key issues and some limitations of the research, and suggest possible future studies.

An emphasis on similarities has been thought to be of benefit to intergroup interventions (Brown & Lopez, 2001). However, as predicted by Social Identity Theory, and as found in a number of studies (see Brown, 1984a, for an overview) the attempt to eradicate 'big differences' (as Jetten & Spears, 2003, put it) comes with the risk of eliminating those valued points that make groups distinct. As the same authors note, both situations that cause low distinctiveness (e.g., presenting or perceiving the ingroup to be similar to the outgroup) and situations that cause high distinctiveness (e.g., presenting or perceiving the ingroup to be different from the outgroup) can bring the same negative outcomes albeit via different processes.

Brown (1988) posited that, at least with regards to similarity induction, it is only high levels of similarity that cause low distinctiveness and therefore distinctiveness threat which, in turn leads to intergroup differentiation. Whereas attempts have been made to manipulate varying levels of similarity (e.g., Roccas & Schwartz, 1993), the general tendency has been to keep differences and similarities apart and never to combine them within the same condition. I attempted to combine similarities and differences in a new manipulation of imagined contact which, in Experiment 4, I compared with a condition that focused solely on differences and a condition that focused only on similarities.

The results of Experiment 4 demonstrated that the imagined contact condition which combined similarities and differences ('balanced similarity' condition) gave rise to more favourable outgroup attitudes in comparison to each of the other two conditions (namely the 'high' and the 'low' similarity conditions), with the last two conditions yielding no differences

in outgroup attitudes. Even more importantly, perhaps, only the difference between the 'balanced' and the 'high' similarity condition on outgroup attitudes was explained by differences in distinctiveness threat. It is worth noting here that distinctiveness threat in this instance pertained to the reaction or the fear stemming from the idea that the two groups, Greek Cypriots and Turkish Cypriots, are all Cypriots, i.e., indiscriminate under one common identity. With the identity 'Cypriot' being the common ingroup identity, what these results suggest more generally is that, at least in this context and with this specific ingroup, interventions targeting a common ingroup identity (see Gaertner & Dovidio, 2000) while undermining subgroup identities might not have positive outcomes (see also Crisp et al., 2006, for similar findings).

. I suggested earlier that the effectiveness of the 'balanced similarity' imagined contact condition could be attributed to the 'careful induction of similarities'. One could, however, pose the question of whether it is indeed the careful induction of similarities that caused these effects or not. As the manipulation checks of Experiment 4 showed, the similarity manipulation was successful. This is a first indication that the induction of similarities did indeed work. Further support for this claim is derived from an additional mediation analysis I computed to test whether perceived similarity mediated the differences in attitudes between the 'balanced' and the 'low' similarity imagined contact conditions. As I expected, perceived intergroup similarity was a significant mediator of this effect⁵⁸. This indicated that the 'balanced similarity' condition led to more positive outgroup attitudes in comparison to the 'low similarity' condition via an increase in perceived intergroup similarity. Therefore I can confidently say that whereas in the 'high similarity' condition the effects of similarity were

⁵⁸ Mediation analysis using bootstrapping revealed that the indirect effect from condition ('balanced' vs. 'low' similarity) to attitudes via intergroup similarity was significant at the 0.05 level (point estimate: 0.49 with a 95% confidence interval of [0.03, 0.54]).

hampered because of feelings of threat, the 'balanced similarity' condition allowed for the beneficial effects of similarity to be manifest.

Another question that I could not address in the first experiment concerns the fact that I manipulated both interpersonal and intergroup similarity in the same condition. Previous research on similarity draws a line between interpersonal and intergroup similarity. Unlike intergroup similarity, the capacity of interpersonal similarity to lead to attraction has not been contested. Diehl (1988) directly tested the differential effects of the two types of similarity and demonstrated that whereas interpersonal similarity led to intergroup attraction, intergroup similarity led to intergroup differentiation. Based on Self Categorization Theory (Turner, Hogg, Oakes, Reicher, & Wetherell, 1987), I believe that the participants, given the context of high group salience in which they were placed, must have behaved according to their ingroup identity. This is partly supported by the finding that even though the 'balanced similarity' and the 'low similarity' conditions differed in both the interpersonal and the intergroup similarity measures, only intergroup similarity, when tested as a mediator of the effect of condition on attitudes, was found to have a significant effect.

It may be the case that references to *interpersonal* similarities and/or differences in the scenarios were overshadowed by the references to *intergroup* similarities and/or differences. Hence, it would be interesting to repeat this study using a more decategorized form of imagined contact, where only mentions of interpersonal similarity are made, and to compare it to a more categorized form of imagined contact like the one I utilised in these studies in order to ascertain the differences between inducing interpersonal similarities and/or differences vs. inducing intergroup similarities and/or differences. Of interest too in such a future study

would be to test the moderating effects of social identification, in order to ascertain whether high identifiers are more threatened by intergroup similarity than are low identifiers.

In Experiment 5, I attempted to compare ‘balanced similarity’ imagined contact with the well-established manipulation of positive imagined contact as well as a no-contact control condition. The results of Experiment 5 showed that the two imagined contact conditions yielded equally favourable outgroup attitudes that were significantly more positive than the attitudes reported in the control condition. However, the ‘balanced similarity’ imagined contact condition, unlike the positive imagined contact condition, also led to higher self-efficacy in comparison to the control condition. The two imagined contact conditions significantly differed on this variable as well as the variable of intergroup anxiety, with ‘balanced similarity’ imagined contact leading to higher self-efficacy and lower intergroup anxiety in comparison to the positive imagined contact condition. I contended that this might be because a realistic emphasis on both similarities and differences in an imagined contact context may be a more ecologically valid instance of an intergroup contact simulation that provides individuals with a mental script for future encounters that is not just positive but also informative. As this is only a conjecture, future research will be needed to investigate which mechanisms explain the differential effects the two types of imagined contact appear to have with regards to self-efficacy and intergroup anxiety.

Another result of Experiment 5 was the significant indirect effect of the ‘balanced similarity’ condition (versus the control condition) on action tendencies via contact self-efficacy and intergroup anxiety combined. Of the specific indirect effects only the mediational effect of contact self-efficacy reached significance, though the effect of intergroup anxiety was large enough to approach significance and should therefore not be neglected. This finding

replicates and extends findings of previous imagined contact studies which found that the effects of imagined contact on behavioural intentions were mediated by intergroup anxiety (e.g., Turner et al., 2009). This study is the first to report an indirect effect of imagined contact on intergroup behavioural tendencies via contact self-efficacy.

The null effects of the positive imagined contact condition on contact self-efficacy and intergroup anxiety may be seen as problematic, for it contradicts on one hand some of the findings of Experiment 3 (see Chapter 3) reported for imagined contact (i.e., intergroup anxiety reduction and a trend for an effect on action tendencies), and the findings of Husnu and Crisp (2010a) showing an imagined contact condition akin to the positive imagined contact condition of this study to yield more positive behavioural intentions amongst Turkish Cypriot participants. In trying to explain these results earlier, I proposed that due to the fact that the positive imagined contact condition in Experiment 5 asked participants to imagine discovering finding out interesting things about the outgroup *and* the outgroup during the imaginary interaction, this might have caused the imagined contact to be a more intergroup than an interpersonal event or that, in other words, it led to higher intergroup salience in comparison to the more individuated scenario adopted by me in Experiment 3 and by Husnu and Crisp (2010a) in their research.

I continued by contending that while higher group salience can be beneficial in terms of the member-to-outgroup generalisation of attitudes, which explains the positive effects of both imagined contact conditions on outgroup attitudes, this may not be the case always for intergroup anxiety. I suggested that perhaps a more individuated interaction (especially amongst participants with high initial levels of intergroup anxiety) might be more helpful for decreasing anxiety. This could mean that different versions of an intergroup contact (e.g., an

imagined contact condition emphasizing individuating information vs. an imagined contact condition emphasizing group information) could, if all else is equal, simply procure positive effects on different intergroup measures. This ‘customization’ of the intergroup contact instance based on what the desired outgroup outcome is has only rarely been attempted before (see Paolini et al., 2007, for an example) but there may be value in further exploring it especially in these cases where intergroup contact is used for intervention purposes.

Lastly, in the interpretation of my results and particularly in the explanation of the null findings, I relied heavily on assumptions with regards to group salience. However, a group salience measure was not included because I did not expect the various conditions to differ on this variable. In retrospect, I note that having had this measure included in the two experiments would have given me valuable insights particularly with regards to the differences between Experiment 5 and Experiment 3 (see previous chapter). I feel that in general what the studies presented in the current chapter were missing were measures tapping the imagined interaction, like the ones employed in Experiments 1 - 3. Uncovering as much information as possible about the interaction will help, in my opinion, to better understand the differential effects of different types of contact interventions (e.g., imagined vs. vicarious contact) as well as within each type of contact.

To conclude, imagined contact has been proposed as an especially valuable intervention in settings where individuals have no prior intergroup experiences and limited opportunities for contact. When contact is to be used as an intervention in these settings one needs to be wary of elements of the contact instance that may be perceived as threatening by individuals (e.g., intergroup similarity). Moreover, a focus only on the pleasantness of the interaction may in some cases be inadequate to prepare individuals for future contact. I argued

that this is particularly plausible for individuals who have minimal or no prior contact. Instead, for individuals in such conflicted settings, a careful emphasis on both interpersonal and intergroup similarities and differences in an imagined contact scenario might have the effect, as demonstrated in Experiment 5, of inclining the individuals to pursue contact via enhancing contact self efficacy and reducing intergroup anxiety.

Chapter 5: Studying the effects of intergroup friendships on prejudice - a longitudinal study

The studies presented so far have been experimental in nature. There are clear benefits attached to the experimental method, e.g., it enables the control of confounding variables and, via randomly allocating participants to the various conditions, it is suited to investigating causal relationships. Despite these benefits, experimental studies have poor ecological validity and their results cannot always be generalised to the wider population as the sample typically utilised in experiments is highly specific. In my experimental studies, for example, I recruited only students as participants, the vast majority of whom were female. As a consequence, little can be said about whether the results of these studies would be observed in the general population of Greek Cypriots and beyond. Furthermore, in the experimental studies described in the previous chapters the emphasis was on contact interventions. Inducing contact, however, is not the same as studying it as it naturally occurs in the outside world. For these two reasons my final study takes the form of a survey that utilises a representative sample of the Greek Cypriot population, thereby enabling me to study contact, and more specifically cross-group friendships, in the real world beyond the laboratory.

Survey studies have been extremely popular in the study of intergroup contact. In Pettigrew and Tropp's (2006) meta-analysis for example, experiments comprised only five percent of the studies included in the analysis while survey and field studies altogether comprised 71 percent of the total number of studies. Of these studies, the overwhelming majority were cross-sectional in design (Christ & Wagner, 2013). Cross-sectional studies represent a relatively straightforward way of collecting large amounts of data which are likely to give an accurate picture of the real world at a given point in time. Unlike experiments they

can overcome external validity issues as contact is studied outside the constrained environment of the lab and the results can be more readily generalised particularly if the sample is representative of the target population.

There are, however, a set of problems attached to the cross-sectional design, two of which are of particular importance to the study of intergroup contact, where two of the most common questions researched to date have to do with whether the direction of the association between contact and prejudice goes from contact to prejudice (i.e., contact reducing prejudice) or from prejudice to contact (avoidance) and which processes underlie (or mediate) the prejudice reduction effects of contact. Cross-sectional studies are not well-suited to answer these questions as they a) cannot determine causation and b) are susceptible to producing biased estimates when mediation is tested. In the next section I will discuss in greater detail the constraints attached to employing the cross-sectional design in intergroup contact research and consequently explain how a relatively new design in the field, the longitudinal design, seems to be buffered against these problems thus rendering it more suitable to study the effects of intergroup contact.

Cross-sectional design and the determination of causation

Cross-sectional data do not allow for inferences to be made about causal relationships. In the context of intergroup contact research this means that one cannot claim with certainty that intergroup contact reduces prejudice, which would be in line with Allport's contact hypothesis. This leaves open the possibility that the contact-prejudice relationship is caused by the 'reverse' path i.e., the path leading from prejudice to contact. Should the 'reverse' path be found, this would mean that less prejudiced people pursue more contact (or that more

prejudiced people avoid it). In the intergroup contact literature, this possibility is referred to as the selection bias problem (Pettigrew, 1998).

In order to tackle selection bias with cross-sectional data, a common strategy applied in the field of intergroup contact research is the comparison of the reciprocal paths (i.e., the path from intergroup contact to prejudice and the path from prejudice to intergroup contact). Pettigrew (1997) recommended comparing the two paths with the use of various statistical techniques, e.g., nonrecursive models with Structural Equation Modeling (Berry, 1984; Kline, 2010). These techniques essentially allow for a test of causality between the target variables by allowing both the path from the predictor to the outcome variable (e.g., contact to prejudice), and the path from the outcome variable to the predictor (e.g., prejudice to contact) to be simultaneously assessed. Nonrecursive models, for example, manage to do that via the introduction of feedback loops (Kline, 2010). In order to test whether both paths are present, the nonrecursive model is compared to two recursive models, one including only the path from the predictor to the outcome variable and the other one including only the reverse path. If the nonrecursive model is of a superior fit to each of the recursive models this means that both of the paths are at play. One can then test the two paths against each other to determine which one is stronger.

Pettigrew's (1997) study constitutes a good example of the use of nonrecursive models with cross-sectional data in intergroup contact research. Pettigrew (1997) examined whether cross-group friendships were associated with affective prejudice and he also wanted to determine the direction of causation. In a first step, Pettigrew added the two paths (path from friendship to prejudice and the reverse path) to a nonrecursive model. He then compared the nonrecursive model to the two recursive models (each containing only one of the paths). The

results showed the nonrecursive model to be a better fit to the data in comparison to the recursive models which meant that both causal paths were present. The relative strength of the two paths was assessed via the comparison of the nonrecursive model with the freely estimated paths to the nonrecursive model where the paths from friendships to prejudice and from prejudice to friendship were constrained to equality. The results showed that the model in which the paths were constrained to equality had a worse fit in comparison to the model in which the two paths were freely estimated. This indicated that the two paths were not equal in their predictive capacity; the 'forward' path from cross-group friendships to affective prejudice turned out to be stronger than the 'reverse' path from affective prejudice to cross-group friendships.

Following Pettigrew's (1997) attempt, a number of recent studies have tested the direction of causation between contact and prejudice using nonrecursive models with cross-sectional data. For example Wagner et al. (2003), using a representative sample of 3560 East and West German youths (Study 2), found a causal effect from cross-group friendships to (less) prejudice towards foreigners living in Germany, but found no evidence for the reverse path from prejudice to friendships. Similar were the findings obtained in their third study where they collected data from 768 East and West German school students. The outgroup in this study were stigmatized foreign groups (e.g., Poles, Turks, and Roma). Wagner et al. (2003) found that outgroup acquaintances and friendships had a strong effect on prejudice that was mediated by the individuals' perceived importance of intergroup contact. This meant that the more intimate outgroup contact the participants had, the more they thought of intergroup contact as important, and the less antipathy they experienced towards stigmatized groups as a result. Importantly, through nonrecursive models they were able to show that prejudice did not

have a reverse effect on importance. It was not clear from their third study whether they had checked the direct reverse paths from prejudice to intimate contact.

Despite their apparent usefulness, nonrecursive models were not spared from criticism. According to Hunter and Gerbing (1982), by introducing feedback loops, nonrecursive models end up simply informing the researcher whether there is a (simultaneous) circular relationship between the two target variables (e.g., contact and prejudice). That would mean contact having an impact on prejudice, that has an impact on contact, that has an impact on prejudice and so on (Christ & Wagner, 2013), all happening at once. This simultaneous (and endless) circular process, Hunter and Gerbing argue, cannot be an accurate representation of real life relationships. Most importantly as I will argue below, a causal relationship presupposes that the causal factor (X) precedes the occurrence of the caused factor (Y). By artificially changing the position of X and Y, as nonrecursive models do, the problem of time order is not resolved. In cross-sectional data X and Y are measured simultaneously and for this reason there is something intrinsically deficient in using cross-sectional data to make claims about relationships that unravel in time.

The longitudinal design as an antidote to the determination of causation problem

The longitudinal design is by default suited for studying causal relationships. A causal relationship connotes an effect of a variable (X) on another variable (Y). In order for this to happen the occurrence of X has to precede the occurrence of Y. In other words X happens first in time. In longitudinal studies measures of the predictor and the outcome variables are taken at (at least) two points in time. This allows for a test of causation whereby the predictor variable X at time t which comes first in the chronological sequence, predicts a variable Y that comes later in time (i.e., time t +1). In this way also, longitudinal designs are able to take the

time element into account as they allow time for causes to have their effects (Gollob & Reichardt, 1987).

Secondly, for any causal relationship to be true no third variable should account for it. Because both variables are measured at both time points in a longitudinal study, this enables an important third variable, namely prior levels of the outcome variable, to be factored in the equation (Gollob & Reichardt, 1987). In this way the researcher can demonstrate that the effect of contact at time t on prejudice at time $t + 1$ is over and above the effect prejudice has on itself overtime.

Lastly, longitudinal designs can easily assess selection bias. Once again, since measures are taken for both target variables (e.g., contact and prejudice) at all time points, the path leading from contact at time t to prejudice at time $t + 1$, as well as the path going from prejudice at t to contact at time $t + 1$ (while controlling for prior levels of each outcome variable) can be tested and contrasted simultaneously without needing feedback loops to achieve that.

Cross-sectional studies and tests of mediation

Research on mediation grew in popularity in the field of intergroup contact when a new line of research focused on unraveling the psychological mechanisms through which contact works (Brown & Hewstone, 2005; Hewstone & Swart, 2011). Despite the recency of this endeavour, the amount of research attempting to shed light on the psychological mechanisms through which contact leads to prejudice reduction is considerable. The overwhelming majority of these studies to date had been cross-sectional (see Pettigrew & Tropp, 2008). Recently, methodological concerns about the usefulness of cross-sectional data when testing for mediation were raised (e.g., Cole & Maxwell, 2003; Judd & Kenny, 2010;

Maxwell & Cole, 2007). Cross-sectional designs are by default unsuitable for studying causal relationships as they (progressively) develop in time. As Ployhart and Vandenberg (2010) put it, an intrinsic problem with using cross-sectional designs for determining mediational processes is that the inferences made are based on the relationships of static variables thus ignoring the time elements involved in this process.

Perhaps the most comprehensive account of the difficulties cross-sectional designs encounter when used for mediational analyses was given by Cole and Maxwell (2003) and Maxwell and Cole (2007). According to Cole and Maxwell (2003): “inferences about causation (and hence mediation) that derive from cross-sectional data teeter on (often fallacious) assumptions about stability, nonspuriousness, and stationarity” (p. 559). Variables are said to have stability when their levels remain the same over time (Kenny, 1979).

Nonspuriousness is present when the relationship between two variables of interest (e.g., the independent variable and the mediator) cannot be explained by a third variable. Stationarity refers to the “unchanging causal structure” (Kenny, 1979, p. 232) of the variables or rather to the assumption that “the degree to which one set of variables produces change in another set of variables remains the same over time” (Cole & Maxwell, 2003, p. 560). If, for example, the path of X from t to time t + 1 is equivalent to the path of the same variable from time t + 1 to t + 2 then these two paths are said to be stationary.

To exemplify using a case from intergroup contact research, cross-sectional studies have found so far that cross-group friendships yield more favourable attitudes and that these effects are at least partially mediated by the psychological mechanisms of affective empathy and intergroup anxiety (see Pettigrew & Tropp, 2008, for a meta-analysis). According to these findings therefore, cross-group friendships are thought to cause a reduction in one’s anxiety

concerning future intergroup interactions and an increase in one's empathy for the outgroup which, in turn, cause more positive attitudes toward the outgroup.

This is a process that entails at least three steps: contact at time t predicts mediators at time $t + 1$, and mediators at $t + 1$ predict prejudice at time $t + 2$. Even though this causal chain can be tested with cross-sectional data, the assumptions of stability, stationarity, and nonspuriousness need to hold true in order for this analysis to produce unbiased estimates. These assumptions, however, can only be tested if the measures are taken at three points in time. In the case of cross-sectional data therefore, none of these assumptions can actually be tested and they are instead simply assumed to hold true. In those cases in which these assumptions are violated though, the estimates yielded are likely to be biased.

Maxwell and Cole (2007) specifically examined whether, and if so to what degree, the study of mediation using cross-sectional tests can produce biased estimates. They sought to find out whether the differences between estimates from cross-sectional and longitudinal studies are so large in practice that they would justify a switch to the more demanding longitudinal studies, or whether these differences are fairly small which would justify the continued employment of cross-sectional designs in the study of mediation. In a nutshell, their results show that cross-sectional designs are prone to yield biased estimates for mediation even when mediation is complete and that the degree of the bias can be so substantial that the applicability of cross-sectional tests of longitudinal mediation is “extremely limited” (p. 39).

Testing mediational processes with the longitudinal design

Longitudinal data, unlike cross-sectional data, enable the researcher to test for the aforementioned assumptions of stability, nonspuriousness, and stationarity. Only when these assumptions are satisfied the estimates of the mediation analysis can be deemed reliable.

Longitudinal data are better at addressing nonspuriousness in comparison to cross-sectional data as they enable the control of prior levels of the outcome variable (be it the mediator or the outcome variable) when the hypothesised causal effects are tested. As argued above, one of the plausible predictors of Y at time $t + 1$ (apart from X) is the prior level of the variable itself (i.e., Y at time t). When previous levels of Y are not controlled for, this may lead to an over- or under-estimation of the paths of the mediation model (Selig & Preacher, 2009).

Furthermore, given that measurements are taken at at least three points in time and that these measurements are equidistant (see Selig & Preacher 2009, for a discussion of time lags), stability and stationarity can also be assessed with longitudinal data (Cole & Maxwell, 2003). When at least three time points are employed then a full longitudinal model can be tested where, for example, contact at Time 1 can predict the mediator at Time 2, and the mediator at Time 2 can predict prejudice at Time 3. It should be noted, therefore, that stationarity cannot be tested in half-longitudinal studies which employ only two waves of measurement. For the assumption of stationarity to hold, Time 1-Time 2 and Time 2-Time 3 within-construct paths should be equivalent.

An example from intergroup contact research would help clarify these points. Swart et al. (2011), who were the first to employ a full longitudinal mediation model for intergroup contact, studied the effects of cross-group friendships on prejudice (attitudes and action tendencies) via two mediators (intergroup anxiety and affective empathy). Their hypothesised model of mediation was that cross-group friendships at Time 1 would predict the mediators at Time 2, and mediators at Time 2 would predict prejudice at Time 3.

Swart et al. (2011) accounted for prior levels of the mediator and the outcome variables while checking for the causal effects from contact to mediators and from mediators

to the outcome variables. They also tested for stationarity by comparing a model where each variable freely predicted itself at the subsequent time point (e.g., contact at time t freely predicted contact at time $t + 1$, and contact at time $t + 1$ freely predicted contact at time $t + 2$) with a model in which the two paths were constrained to equality. Stationarity would hold if the model where the paths were constrained to equality did not have a worse fit in comparison to the model where the paths were freely estimated. Given stationarity and that the causal paths from contact to mediators and from mediators to prejudice were significant after controlling for prior levels of the outcome variables (i.e., mediators and prejudice), Swart et al. managed to show longitudinal mediation whereby contact at Time 1 predicted Time 2 mediators, and mediators at Time 2 predicted less prejudice at Time 3.

Studying the longitudinal effects of contact on prejudice

As discussed in the previous pages, longitudinal designs constitute a useful tool for the study of directionality of the contact-prejudice effects and the study of mediational processes explaining the effects of contact on prejudice. An obvious additional research question longitudinal designs are undoubtedly most suited to address is whether the effects of intergroup contact are short-lived or whether they endure in time. In the experimental studies reported earlier (Experiments 1-3), for example, the effects of contact on all the variables apart from intergroup anxiety faded away a week after intervention. Experiments aside, in field surveys too, effects that exist at one point in time, may turn out not to hold in time either because they are not strong enough or because of a number of different interpersonal (e.g., nature of cross-group relationships changing) and/ or contextual (e.g., intergroup relations worsening) reasons.

Longitudinal studies have been on the rise in the field of intergroup contact especially in the last decade. Researchers have typically been employing two-wave studies where the variables of interest are measured at two points in time which are typically separated by a time-lag of some months. The data tend to be analysed using the cross-lagged approach with either observed or latent variables (e.g., Binder et al., 2009, Brown et al., 2007, Eller & Abrams, 2003; 2004, Levin et al., 2003). I will provide a brief explanation of this approach using Structural Equation Modeling before moving on to present the actual studies.

The idea underlying the cross-lagged approach is that the basic model of change that explains the longitudinal data is the auto-regressive model of change, according to which the levels of a variable Y at a later time ($t + 1$) can be predicted at least to some extent by the value of the same variable Y at a previous time point (t) (Maxwell & Cole, 2007). Given that this is the case (i.e., if the autoregressive model provides an acceptable fit to the data) then the question of interest is whether the value of a variable other than Y (say X) at time t can also predict Y over and above the effect Y has on itself at time $t + 1$. The paths going from X at an earlier point to Y at a later point are called cross-lagged paths. The autoregressive plus the cross-lagged paths together comprise the cross-lagged model of change according to which the temporal change can be explained by both the autoregressive and the cross-lagged paths. Since the cross-lagged model and the autoregressive model are nested⁵⁹, the two can be

⁵⁹ Two models are nested in relation to each other when one is a slightly different version of the other and they differ in degrees of freedom. Each extra path set to be calculated results in a reduction of degrees of freedom (one degree of freedom per path). If, for example, two cross-lagged paths are to be calculated (e.g., Time 1 Contact to Time 2 Prejudice, and Time 1 Prejudice to Time 2 Contact) then the cross-lagged model which includes these paths (along with the autoregressive paths) will have a difference of two degrees of freedom in comparison to the autoregressive model (which only includes the autoregressive paths). These two models can be compared to each other in terms of their fit to the data. This is done with the help of statistical tests (e.g., chi-square difference test). A significant difference in chi-square will indicate that the nested model has a better fit in comparison to the model to which it is nested and it should therefore be adopted.

compared with each other using specific statistical tests (see Satorra & Bentler, 1999, for difference chi-square test statistic).

Next I will review the longitudinal studies published to date exploring the temporal effects of contact on prejudice. In those cases in which the ‘reverse’ path was tested, it will be mentioned. I will then present the findings of longitudinal studies on the mediation of the temporal effects of direct contact on prejudice. Lastly, I will discuss the very few longitudinal studies that assessed the temporal effects of extended contact on prejudice and their mediation before proceeding to present Study 6. It should be noted here that even though I use the terms ‘direct contact’ and ‘cross-group friendships’ interchangeably, the vast majority of the studies reported here assessed friendships and not mere contact (as in acquaintances).

The longitudinal effects of direct intergroup contact

Despite longitudinal studies being a rather recent trend in the area of intergroup contact there have been attempts to capture the longitudinal effects of contact on prejudice early on. Stephan and Rosenfield (1978), for example, conducted a two-year longitudinal study on approximately 230 white American school children aiming to assess the various effects of desegregated schooling. They explored how changes in interethnic contact (among other predictors) predicted changes in the attitudes of White students towards Blacks and Mexican Americans. They demonstrated through cross-lagged correlations that, in line with the contact hypothesis, contact at Time 1 was related to attitudes at Time 2, whereas attitudes at Time 1 were not related to contact at Time 2.

In one of the first longitudinal studies conducted since Stephan and Rosenfield’s study and one of the very few studies incorporating more than two time points, Levin et al. (2003) collected data from students of four different ethnic groups (White, Asian, Latino, and African

American) studying at an American college. The four measurement points took place at the end of each of the four years at college and of the approximately 2000 respondents that responded to the first questionnaire, 1215 provided responses on all four occasions. Levin et al. were interested in capturing the relations between ingroup bias, cross-group friendships and intergroup anxiety over time. More specifically, they sought to find out how Time 1 levels of ingroup bias and intergroup anxiety affected students' choice of friendship at subsequent time points, and how friendships in their turn affected intergroup anxiety and intergroup bias at Time 4. They found (akin to the reverse path from prejudice to contact) that less intergroup anxiety and ingroup bias at Time 1 led to more outgroup friendships at Time 2 and Time 3. In line with the contact hypothesis, they found that more outgroup friendships at Time 2 and Time 3 were associated with less anxiety and less ingroup bias at Time 4.

Utilizing a two-wave design, Eller and Abrams (2004) assessed the longitudinal effect of direct contact on prejudice in two studies. In their first study a matched sample of 34 first-year English undergraduate students reported the amount (frequency of contact) and the quality (contact as close friends) of contact they had with French students over two time points separated by a 6-month lag. They found that contact at Time 1 led to a Time 1-Time 2 change in the levels of intergroup anxiety and of outgroup evaluations such that the more contact participants reported at Time 1 the lower their anxiety and the more favourable their outgroup attitudes were at Time 2 (in comparison to Time 1).

It should be noted, however, that it was only when participants reported contact as being of good quality (i.e., cross-group friendships) that contact led to less prejudice at Time 2 (in comparison to Time 1). There was no effect of contact quantity on a Time 1-Time 2 change in outgroup evaluations. Instead, there was a significant positive effect of contact

quantity on subsequent anxiety levels whereby more contact at Time 1 lead to higher anxiety at Time 2 (in comparison to Time 1). Similar results were found in their second study, where they found that Mexicans (N = 67) working in multinational organizations in Mexico who reported having good quality contact with Americans at Time 1 reported less social distance and more favourable attitudes towards Americans at Time 2 in comparison to Time 1. Conversely, no effects were found for contact quantity on later levels of prejudice. They also found significant effects for the reverse indirect path from Time 1 anxiety to Time 2 outgroup friendships via affective ties⁶⁰. Higher anxiety at Time 1 led to greater Time 1-Time 2 changes in affective ties (worse affective ties) and (worse) affective ties predicted, in turn, fewer outgroup friendships at Time 2 in comparison to Time 1.

Brown, Eller, Leeds, and Stace (2007), in a two-wave longitudinal study with 109 students at a secondary state school (mean age = 12.2 years), tested whether interschool contact (quantity and quality) had an influence on the participants' prejudice (desired closeness, negative evaluation, and inhumanization) towards private school students. The reverse paths from Time 1 desired closeness, negative evaluation, and inhumanization to Time 2 contact (quality and quantity) were also tested. They found that the amount of contact the participants reported having with a member of the outgroup (frequency of contact) significantly predicted more desired closeness with, less negative evaluation and lower inhumanization of, the outgroup at Time 2 while controlling for Time 1 levels of the dependent variables. Interestingly, they did not find any significant effects for the effect of prejudice on contact. Also, in contrast to Eller and Abrams (2004) they did not find significant prejudice reduction effects for quality of contact.

⁶⁰ Affective ties were operationalized as the Inclusion of Other in the Self (IOS) (see Aron et al., 1992).

A larger scale longitudinal study ($N = 1655$) that explicitly tested the direction of the causal processes between contact and prejudice was conducted by Binder et al. (2009) who tested ethnic minority and ethnic majority school children's cross-group friendships and their prejudice towards their respective ethnic outgroups. They collected data from Germany, Belgium, and England over two time points, separated with a lag of seven months. They were interested in the direct friendships' effects on negative outgroup emotions and desire for social distance. The reverse path was simultaneously tested and compared to the forward path.

Their overall results showed a bidirectional relationship between friendships and prejudice, such that negative outgroup emotions and desire for social distance at Time 2 were predicted by the amount of reported friendships at Time 1, controlling for prior levels of the dependent variables. The higher the number of outgroup friendships at Time 1 the lower the prejudice was at Time 2. The reverse path was also significant. The more negative outgroup emotions and the more social distance the participants reported at Time 1, the less friendships they reported at Time 2 controlling for Time 1 friendships. A comparison of the forward and the reverse paths indicated no difference between them apart from one case: the effect of Time 1 social distance on Time 2 friendships was stronger than the path from Time 1 friendships to Time 2 desire for social distance.

Dhont et al. (2011) in a two-wave longitudinal study, asked 65 young Belgian adults to rate the amount of intergroup contact they had with non-European immigrants. The contact items asked how many immigrant friends they had and how often they had contact with immigrants in their circle of friends. Measures were also taken on subtle racism, outgroup attitudes, and endorsement of negative stereotypes. Cross-lagged analyses yielded a

significant effect of Time 1 friendships on Time 2 prejudice but, like Brown et al. (2007), they found no reverse effect from Time 1 prejudice to Time 2 friendships.

In summary, all longitudinal studies reported to date found a significant temporal effect of direct friendships on prejudice (reduction)⁶¹. Importantly, these effects did not cease to exist even once prior levels of prejudice were taken into account. The findings regarding the 'reverse' paths are less consistent. Brown et al. (2007) and Dhont et al. (2011) found no effects of prejudice on contact. Eller and Abrams (2004) as well as Levin et al. (2003) who found both paths to be significant, did not attempt a comparison between them. The only study reporting the existence of both paths that also performed a comparison between them was the study of Binder et al. (2009). They found the two paths not to differ apart from one case: the path from social distance at Time 1 to contact at Time 2 was stronger than the path from contact at Time 1 to social distance at Time 2.

Mediation of the effects of cross-group friendships on prejudice in longitudinal studies

Two of the aforementioned two-wave studies investigated the mediational effects of third variables that have been found by cross-sectional and experimental studies to significantly mediate the effects of contact on prejudice. Eller and Abrams (2004) tested whether inclusion of other in self, learning about the outgroup, and ingroup reappraisal, mediated the effect of intergroup contact on prejudice. More specifically they tested whether Time 1 contact led to a Time 1-Time 2 change in the mediators which then led to less prejudice. In Study 2 they found that contact at Time 1 caused a Time 1-Time 2 change in

⁶¹ Results are more inconclusive for the effect of direct contact (not friendships) on prejudice reduction over time. Contact quantity was found to be positively related to intergroup anxiety in Eller and Abrams's (2004) study unlike contact quality that led to overall prejudice reduction in the same study. In contrast, Brown et al.'s (2007) study found contact quantity and not quality to have prejudice reduction effects overtime. This might be because the measure of contact quality differed in the two studies. In Eller and Abrams' study the quality items essentially tapped friendships ('to what extent do you consider the outgroupers you have contact with as close friends'), and therefore one could argue that this might have constituted a friendship and not a quality of contact item.

inclusion of other in self which then predicted intergroup anxiety, social distance, as well as positive evaluations of the outgroup at Time 2.

Testing intergroup anxiety as a mediator of the temporal effects of direct friendships on prejudice, Binder et al. (2009) found that intergroup anxiety significantly mediated the effects of both the amount of direct friendships and the quality of direct friendships on negative emotions and social distance. Instead of calculating a change score for the mediator as Eller et al. (2004) did, Binder et al. (2009) inserted both Time 1 and Time 2 intergroup anxiety scores as predictors alongside Time 1 friendships and Time 1 prejudice. They argued that controlling for the mediator as well as the criterion variable at Time 1 constituted a very strict test of mediation⁶².

Despite the methodological advancements made in assessing mediation within longitudinal studies, there are still concerns voiced about the utility of two-wave studies when it comes to making claims for the mediation of longitudinal effects. Studies with three- or more waves on the other hand, are deemed to be ideal for the study of mediational processes partly because they can account for stationarity, one of the assumptions thought to hold true when mediational effects are estimated. As far as I am aware, the study by Swart et al. (2011) is the only longitudinal study published so far focusing on the mediation of intergroup contact's effects on prejudice that incorporated more than two measurement points.

⁶² Whether it is a 'very strict test of mediation' or whether Binder et al.'s way of testing mediation in a two-wave design is the prescribed way to go about it, is still arguable. According to Cole and Maxwell (2003) in the case where the three variables, X (e.g., intergroup contact), M (e.g., intergroup anxiety), and Y (e.g., prejudice) are measured at both points in time then the recommended way for testing mediation is to conduct two longitudinal tests, one to estimate path a (i.e., the path from X to M) and one to estimate path b (i.e., the path from M to Y). For estimating path a, one needs to estimate the path from X at Time 1 to M at Time 2 controlling for M at Time 1, and for path b one needs to estimate the path from Time 1 M to Time 2 Y controlling for Time 1 Y. This test resembles much more the standard way of testing for mediation which is precisely the estimation of the paths a and b.

Swart et al. (2011) employed a full longitudinal mediation design (see Selig & Preacher, 2009) in their assessment the effect of cross-group friendships on positive outgroup attitudes, perceived outgroup variability, and negative action tendencies, and the mediation of these effects by affective empathy and intergroup anxiety. They hypothesized that Time 1 and Time 2 contact would predict Time 2 and Time 3 mediators respectively, and that Time 1 and Time 2 mediators would predict Time 2 and Time 3 outcome variables respectively. Given stationarity between the Time 1-Time 2 and Time 2-Time 3 autoregressive and cross-lagged paths, this would indicate mediation where Time 1 contact predicts Time 2 mediators, which in turn predict Time 3 prejudice. They also expected the reverse paths to be significant which led them to predict that the model best describing their data would be a bidirectional model where Time 1 contact would predict Time 3 prejudice via the two Time 2 affective mediators, while Time 1 prejudice would also predict Time 3 contact via the mediators at Time 2.

Indeed, the bidirectional model was a better fit to the data in comparison to both the unidirectional ‘forward’ and ‘reverse’ models. Cross-group friendships predicted both intergroup anxiety and affective empathy at subsequent time points. Of the two mediators, intergroup anxiety predicted perceived outgroup variability at later stages, whereas affective empathy predicted all outcome variables at subsequent time points. This meant that the longitudinal effects of cross-group friendships on prejudice were mediated by both intergroup anxiety and affective empathy. The reverse paths showed that there was no reverse longitudinal mediation of the effect of prejudice on cross-group friendships, but that there was an effect from prior levels of positive outgroup attitudes and negative action tendencies to later levels of affective empathy. There was also a significant effect from intergroup anxiety to subsequent levels of cross-group friendships.

Longitudinal effects of extended friendships and their mediation

So far, findings from experimental (e.g., Liebkind & McAlister, 1997), quasi-experimental (Cameron et al., 2011), and cross-sectional (e.g., Gomez et al., 2011) studies provided ample support for Wright et al.'s (1997) assertion that extended contact, just like direct contact, can reduce prejudice. While the call was heeded for more longitudinal studies to be conducted on the temporal effects of direct friendships, less work has been done on the longitudinal effects of extended friendships on prejudice reduction in comparison to the longitudinal study of the effects of direct friendships.

Christ et al. (2010, Study 2) interviewed 984 Catholics and Protestants living in mixed and segregated neighborhoods in Belfast. They interviewed the same individuals again after a period of approximately one year. Slightly fewer than half of the participants ($N = 404$) responded to both measures. One of the research questions pertained to whether extended friendships had a temporal effect on two dependent variables, positive behavioural intentions towards the outgroup and attitude strength. Christ et al. found extended contact at Time 1 to predict behavioural intentions and attitude strength at Time 2 while controlling for prior levels of the two outcome variables. They also hypothesized that the longitudinal effects of direct friendships in comparison to the effects of extended friendships on attitude strength would be more profound. The results did not provide support for their hypothesis.

Importantly, the longitudinal effects of extended contact were moderated by residential location. Participants living in segregated neighborhoods where opportunities for direct contact were low benefited from having extended friendships whereas individuals' extended friendships did not make any difference for people living in mixed neighborhoods who had more opportunities for direct contact. Differences in the effects of extended contact for

individuals coming from segregated versus mixed neighborhoods were, as hypothesized by Christ et al. (2010), accounted for by levels of direct friendships (mediated moderation). Thus, extended friendships had a prejudice reduction effect over time only for individuals coming from segregated (as opposed to mixed) neighborhoods, and this difference was due to the fact that individuals from segregated neighborhoods had less direct friendship with the outgroup.

While Christ et al. (2010) focused on the direct effects of extended friendships on prejudice (and their moderation by direct contact), the other two longitudinal studies currently published on extended friendships looked at the mediation of the effects of extended friendships. Eller et al. (2011) tested in a quasi-experimental longitudinal study whether any effects of (perceived) extended friendships on intergroup bias were mediated by reduced ignorance, increased awareness of outgroup positive exemplars (outgroup norms), and reduction of intergroup anxiety. Eller et al.'s (2011) study had two conditions, the direct and the extended contact conditions. The direct contact condition was made up of international students who studied for one year in the U.K. and who therefore had the opportunity to come into face-to-face communication with British students. The extended contact condition was comprised of friends of the first group of students, who remained in their home countries throughout the duration of their friends' stay in the U.K. The idea was that the group of students having direct contact with the outgroup (direct contact group) would report back to their home friends (extended contact group) on the friendships they developed throughout their stay in the U.K. In their report of this study, Eller et al. (2011) focused specifically on the effects of extended contact on intergroup bias and its mediation.

To measure perceived extended contact, the extended contact group was asked how many British friends they estimated their friends who were studying in the U.K. had. Their

own direct contact with British students was assessed and controlled for by asking them how many British people they knew who they would count as friends. They were also asked how ignorant they considered themselves to be with regards to their knowledge of the outgroup (self-perceived ignorance about the outgroup), how they would feel if they were asked to interact with British people (intergroup anxiety), how they perceived the behaviour of British people towards their ingroup, i.e., their friend (perceived outgroup behaviour), how close they perceived their friends to be with their British friends (perceived inclusion of other in self) and finally, they were asked to evaluate their feelings towards the outgroup in general. These measures were taken twice, once at the beginning of the visit of their friends to Britain (Time 1), and once at the end of their journey, approximately a year later (Time 2).

In their analyses Eller et al. assessed the effects of the change over-time in perceived extended contact on changes over time in the mediators (i.e., Time 1-Time 2 differences in self perceived ignorance, perceived inclusion of other in the self, intergroup anxiety, and perceived outgroup behaviour), and Time 2 levels of the outcome variable (outgroup evaluation) while controlling for direct friendships and Time 1 levels of the outcome variable. The results yielded a direct effect of change in perceived extended friendships on Time 2 outgroup evaluation which was partially mediated by a change in outgroup norms (i.e., perceived outgroup behaviour). While changes in perceived extended contact predicted changes in self-perceived ignorance and perceived inclusion of other in the self, perceived inclusion of other in self did not predict, in turn, the outcome variable at Time 2. Also, whereas changes in perceived extended contact had no effects on changes in intergroup anxiety levels, anxiety levels had an effect on outgroup evaluation. Participants whose

intergroup anxiety became less over this one year, reported having a greater liking for the outgroup.

I should note here that even though Eller et al. (2011) conceived and presented their study as a longitudinal test of the effects of extended contact and their mediation, their analyses do not justify this claim. As I outlined earlier, Eller et al. (2011) looked at how the Time 1-Time 2 change in perceived extended contact affected the Time 1-Time 2 changes in the mediators. A change score predicting another change score does not yield a longitudinal analysis, as the cause, X (i.e., changes in perceived contact) does not come before M (changes in the levels of the mediator) in time. They preserved on the other hand a longitudinal element in their analyses by checking how changes in the mediator affect Time 2 prejudice by controlling for Time 1 prejudice.

Even though it is not clear to me why Eller et al. (2011) utilized change scores for their independent and mediating variables while they controlled for Time 1 levels for their dependent variable, I suspect that what they were trying to apply essentially was a half-longitudinal design (see Cole & Maxwell, 2003) within which the longitudinal relation between M (mediator) and Y (outcome variable) is assessed while only examining the contemporaneous (i.e., cross-sectional) relationship between X (independent variable) and M (mediator). The problem with such a design is that one cannot claim with certainty that X causes M as the two are treated as measures that were obtained concurrently. I would therefore propose that despite having included this study as an instance of a longitudinal study, this study is different from the rest of the longitudinal studies described so far and

therefore comparisons between its results and the rest of the longitudinal results should be made with some caution⁶³.

Feddes et al. (2009) compared the longitudinal effects of direct and extended friendships on outgroup attitudes and the mediation of these effects by social norms. They also tested whether the path from prejudice to (either type of) contact was significant. They recruited 202 German and Turkish children studying in German schools and interviewed them at two points separated by a seven-month lag. One hundred and forty-nine children responded to both questionnaires. They found Time 1 direct friendships leading to more positive outgroup evaluations at Time 2 (while controlling for Time 1 outgroup evaluations) only for the majority group (German children). The reverse path was also assessed and was found not to be significant. There were no effects of Time 1 extended friendships on Time 2 outgroup evaluations for either the majority or the minority group. Furthermore, the effect of direct friendships on outgroup evaluation was found to be mediated by intergroup norms about cross-group friendships. Time 1 direct friendships led to the belief that cross-group friendships were not negatively sanctioned by either the ingroup or the outgroup, and this in its turn predicted more positive outgroup evaluations at Time 2.

It should be mentioned here, however, that while extended friendships yielded no longitudinal effects on the prejudice measure, the cross-sectional intercorrelations between

⁶³ I also want to note here that at least one prior study I am aware of that aimed like Eller et al. (2011) to test the effects of a contact intervention on outcome variables and their mediation using the same pretest-posttest design, did end up treating the data as cross-sectional. Specifically in their study, Al Ramiah, Hewstone, Little and Lang (2013) tested the effects of a nation-building intervention program in Malaysia in terms of the effects of contact on outgroup evaluation and their mediation by intergroup anxiety and perceived threat. What Al Ramiah et al. (2013) did was to examine whether post-intervention contact led to lower post-intervention anxiety and lower post-intervention symbolic threat, and whether lower post-intervention intergroup anxiety and symbolic threat in their turn led to more positive post-intervention outgroup evaluation while *controlling* for the pre-intervention levels of all these variables. The discrepancy in the way the two studies analyzed data coming from the same design raises the question of how to go about assessing mediation in studies including intervention. Even though I thought this a point worth noting, I will not elaborate further on it, as this is a question going beyond the scope of this thesis.

extended friendships and prejudice were significant at both time points. Extended friendships were also found to be significantly related to the mediators, social norms, at Time 2 (but not at Time 1). When the time component was taken into account, however, the effects of extended friendships on both outgroup evaluation and social norms were non-significant. Though this is not an easily interpretable result, what it shows in my opinion is that there is some association between extended friendships and norms as well as outgroup evaluations, but that a strict longitudinal test could not provide evidence that extended friendships caused more favourable social norms or more favourable outgroup evaluation.

Summarizing the above, even though the number of longitudinal studies available on extended friendships is small and does not allow for any safe conclusions to be drawn, there is some evidence that extended friendships just like direct friendships have temporal effects on prejudice. This is the case especially in contexts where opportunities for direct contact are scarce and individuals have low direct contact themselves (Christ et al., 2010, Study 2). This is a finding that replicates cross-sectional findings on the moderating role of (high quality) direct contact on extended friendships' effects (see Cameron et al., 2011; Christ et al., 2010, Study 1). High levels of (high quality) direct contact can undermine any extended contact effects. However, in those cases in which extended contact comprises the primary source of information about the outgroup, i.e., in low direct contact contexts, then its effects manifest themselves and can be rather profound. Finally, the mediation of the longitudinal effects of extended friendships on prejudice remains significantly underexplored. Of the two studies addressing longitudinal mediation for extended friendships, the study by Feddes et al. (2009) found no direct effects to be mediated, but they also found no temporal effect of extended friendships on the mediating variable, social norms. Eller et al. (2011), on the other hand,

found support for the mediating role of self perceived ignorance, perceived inclusion of other in the self, and outgroup norms, but not intergroup anxiety.

Study 6: A comparison of the longitudinal effects of extended and direct friendships and their mediation in a low-contact context

The growing interest in the study of the longitudinal effects of intergroup contact resulted in a fair number of longitudinal studies conducted on the temporal effects of direct contact (and especially cross-group friendships) on prejudice. Less attention has been paid so far to the study of the longitudinal effects of extended friendships and even fewer studies tested direct and extended friendships simultaneously (for an exception see Christ et al., 2010). Given the importance attached to alternative types of contact, especially in contexts where direct contact is not the norm or even a possibility, it is important to assess whether alternative types of contact, and more specifically extended friendships, have an enduring effect on prejudice. It is also of interest to compare the effects of these two types of contact and how these effects are brought about in such a context. As little has been done on this front, a number of theoretically interesting questions remain to be addressed.

Firstly, the only comparison of the longitudinal effects of direct and extended friendships by Feddes et al. (2009), was performed according to the researchers in an ethnically heterogeneous context which provided multiple opportunities for good quality direct contact. As could be expected, in this ethnically diverse context which provides ample opportunities for cross-group interactions, the contribution of contact to prejudice reduction is more likely to be explained by direct friendships than extended friendships. We know by now from the studies of Christ et al. (2010) and Cameron et al. (2011) that the effects of extended

friendships are likely to be more pronounced and therefore traceable in contexts where scarce opportunities for intergroup contact lead to very little direct contact.

What is not clear from these studies' findings is how extended friendships would fare in comparison to direct friendships in contexts of high segregation and low contact. While the most plausible hypothesis is that direct friendships will have the most profound effects, there is also a possibility that extended friendships might yield effects that are at least as profound. Turner et al. (2008), for example, found the effects of extended friendships to be more pronounced than the effects of direct friendships in their study (although they did not directly compare them) and they attributed this finding to the fact that the research context was one of low (direct) contact. There is a likelihood therefore, that in extremely segregated contexts, knowing other people who have outgroup friendships can turn out to be at least as important as having one's own direct friendships.

A second theoretical question pertains to whether extended friendships, like direct friendships, can also be subject to selection bias. Wright et al. (1997) contended that since people cannot control who their friends are going to be friends with, this means that selection bias is rather unlikely to occur for extended friendships. Eller et al. (2011), who adopted this opinion, took the conscious decision not to test for the existence of reverse paths (i.e., from prejudice to changes in perceived extended friendships) in their study. This is, however, a rather weak argument that has been contested by other researchers. For example, De Tezanos-Pinto et al. (2010) argued that even though we do not choose who the friends of our friends will be, we do choose our own friends to begin with. Indeed, according to the similarity attraction hypothesis (Byrne, 1971), we are likely to choose our friends based on how similar they are to us in terms of values and shared world views among other things.

The implication of this for extended friendships is that an individual who is more tolerant and holds overall less prejudiced views about social groups other than their own, would be more likely to have ingroup friends with similar values, who in turn, would be more likely to have cross-group friendships. This provides a good argument for why the reverse path from prejudice to extended friendships should not be ignored and should also be tested. I would add to the above that testing for the selection bias, especially in highly segregated contexts, is of great importance. If contact is not the norm and one has to actively pursue it, then the possibility that less prejudiced individuals are the ones who end up having direct and/or extended intergroup contact cannot be neglected.

Finally, a third question of theoretical importance is whether the two types of contact give rise to more positive attitudes (and less prejudice in general) via the same or different mechanisms. Cross-sectional, experimental, and longitudinal research investigating the mediators of the effects of direct contact on prejudice has almost unequivocally⁶⁴ given support to the mediational role of intergroup anxiety. Intergroup anxiety, as was hypothesized by Wright et al. (1997), turned out to be one of the underlying mechanisms explaining the effects of extended contact too (e.g., Gomez et al., 2011; Paolini et al., 2004). Whereas longitudinal research is in agreement with cross-sectional and experimental research with regards to the mediating role of intergroup anxiety between direct friendships and prejudice, the only longitudinal study available that tested intergroup anxiety (reduction) as a mediator for extended friendships, that by Eller et al. (2011), failed to find this indirect effect. Even though they found a path leading from changes in intergroup anxiety to outgroup attitudes at Time 2, there was no path linking extended friendships to changes in intergroup anxiety.

⁶⁴ For a notable exception see Turner et al. (2008).

Social norms (ingroup and outgroup norms) were conceived to be more apt to explain how extended friendships lead to prejudice reduction (Davies, Wright, Aron, & Comeau, 2013). There are a number of studies by now providing support for the role of norms in explaining how extended contact works (e.g., Cameron et al., 2011; Eller et al., 2011; Gomez et al. 2011; Turner et al., 2008). However, the only longitudinal study aiming to check the mediation of the temporal effects of extended friendships on attitudes via social norms (see, Feddes et al., 2009) failed to find support for a temporal effect of extended friendships on norms.

Even though there were no hypotheses as to whether changes in norms would mediate the effects of direct friendships on prejudice, results from both cross-sectional (e.g., Carlson, Wilson, & Hargrave, 2003) and longitudinal (Feddes et al., 2009) studies provide support for the mediational function of norms for direct friendships too. Davies et al. (2013) suggest two possible explanations for this effect. They propose that direct friendships can lead to a change of ingroup norms because the individual who has outgroup friends knows from his or her own experience that their outgroup friendships had not been sanctioned by the ingroup or the outgroup, as neither themselves nor their friend were ostracized by their respective ingroups. This may lead individuals to the inference that their and their friend's respective ingroups do not hold negative views about intergroup interactions. The second explanation proposed by Davies et al. (2013) is particularly plausible when the norms of the group are not that clear and therefore one has to use one's private experiences to infer knowledge about the group (and its norms). Drawing from the notion of self-anchoring (e.g., Otten & Bar-Tal, 2002; Otten & Epstude, 2006), Davies et al. (2013) argued that since our ingroup is part of who we are, there is a likely overlap in the representations we have about ourselves and the

representations we have about our ingroup. Therefore, when unsure about what the prevailing social norms are, the individuals who have direct friendships will use their own positive stance towards the outgroup as a basis to infer positive norms for the ingroup as a whole.

There are not many studies that looked into differences in the underlying mechanisms explaining the effects of each type of contact. Turner et al. (2008) were the first, as far as I know, to undertake this task. Turner et al. investigated whether, intergroup anxiety, ingroup, and outgroup norms, and inclusion of other in one's self, mediated the effects of direct and extended friendships on attitudes. They found that the effect of extended friendships on attitudes was mediated by all four mediators, unlike direct friendships whose effects were only mediated by inclusion of other in self. Similarly, the study by De Tezanos-Pinto et al. (2010) specifically compared the effects of direct and extended friendships on prejudice as well as whether the mediational processes via which they yielded their effects on prejudice differed. They hypothesized that ingroup norms about the social approval of intergroup contact would differentiate the two types of friendships. In line with their hypotheses, they found that the effects of direct contact were only mediated by the reduction of intergroup anxiety and not by a change in ingroup norms, unlike extended friendships whose effects were mediated by both mediators.

Finally, an unpublished study by Tam, Hewstone, Cairns, and Geddes (reported in Vonofakou, Hewstone, Voci, Paolini, Turner, Tausch, Tam, Harwood, & Cairns, 2008) tested self-disclosure, peer out-group attitudes (ingroup norms), and empathy as possible mediating variables of the effects of direct and extended friendships. They found different mechanisms underlying the two types of friendships; the effects of extended friendships were mediated by

the perceptions of peer attitudes towards the outgroup (perceived them to be more positive), whereas direct friendships worked via self-disclosure.

As mentioned already, the only longitudinal research that explicitly contrasted the effects of direct and extended friendships and their mediation, the study by Feddes et al. (2009), found a longitudinal mediation of the effects of direct friendships via norms, but no direct effects of extended friendships on norms. Interestingly, they found correlations between extended friendships at Time 1 and ingroup norms at Time 2 thus suggesting that there is not a complete absence of a relationship between extended friendships and norms but that probably this relationship is not (yet) strong enough to appear longitudinally.

It is noteworthy that the longitudinal studies so far have yielded results that contradict the vast majority of cross-sectional findings. Apart from the study of Feddes et al., Eller et al. (2011) failed to find support for the mediating role of intergroup anxiety between extended contact and prejudice. Due to the scarcity of research on the mediation of the longitudinal effects of extended friendships, and the fact that the results so far are inconclusive and at times in disagreement with cross-sectional results, more research in this area will contribute to our knowledge concerning the longitudinal mediation of extended friendships and how it differs, if it does, from the longitudinal mediation of direct friendships.

Present study

This study sought to address the aforementioned questions by employing a three-wave longitudinal design where the key variables, the predictors (extended friendships and direct friendships), mediators (intergroup anxiety and ingroup norms), and the outcome variable (outgroup attitudes) were measured at three points in time. For this purpose a representative

sample (N = 1000) of the Greek Cypriot community living in Cyprus were interviewed at three points in time over the course of one and a half years⁶⁵.

Hypotheses

The first theoretical question pertained to the comparison of the effects of direct and extended friendships on prejudice. As the longitudinal effects of direct friendships on prejudice reduction have been uncontested (e.g., Binder et al., 2009; Dhont et al., 2011), I predicted that both direct friendships and extended friendships would predict less prejudice (operationalized in this study as -more favourable-attitudes) overtime (Hypothesis 1). Despite Feddes et al. (2009) finding no longitudinal effects for extended friendships, based on the results of the two studies by Christ et al. (2010), I expected that extended friendships would be effective in reducing prejudice in a highly segregated context like Cyprus.

I had no firm hypotheses as to whether direct friendships would turn out to have stronger effects than extended friendships as the longitudinal effects of the two types of friendships within a low-contact environment have never been compared in the past. Even though Turner et al. (2008) reported more pronounced cross-sectional effects for extended rather than direct friendships in their study of the effects of contact in a low-contact context, there is a general consensus that direct friendships, as the most direct and intimate form of contact, will still have positive effects on outgroup attitudes even when the effects of extended friendships are pronounced. Therefore, I did not expect the effects of direct friendships to be impeded by the effects of extended friendships. In fact, I expected that if there were any

⁶⁵ I was granted access to this dataset by Dr. Charis Psaltis (University of Cyprus) who was the principal investigator of the projects and Prof. Miles Hewstone (University of Oxford) who was a partner in the first project. The first project entitled “Representations of History and Intergroup Relations in Cyprus” was funded by EU Commission Grant: Cypriot Civil Society in Action, and covered the costs of data collection for the first two waves. The third wave was completed with funds from the UNDP-ACT for the project: “Multiperspectivity and Intercultural Dialogue in Education”.

differences between the two types of friendships, they would be in favour of direct and not extended friendships (Hypothesis 2).

With regards to the second theoretical question, i.e., whether the reverse paths from prejudice to each type of contact would be significant, I expected that friendships would be predicted by prior attitudes. I based this assumption on the fact that contact in the Cypriot context is highly effortful, it rarely happens by chance, and it has to be pursued. As a result, I expected that less prejudiced people would have less intergroup anxiety and more positive ingroup norms and consequently more friendships. In contrast to Wright et al.'s (1997) assertions, I predicted that extended, just like direct, friendships would be influenced by individuals' prior attitudes (Hypothesis 3). In the case in which both paths were significant I was very interested to examine the relative strength of each path. Results from previous studies are inconsistent with regards to the existence of the 'reverse' paths and their relative strength in comparison to the 'forward' paths, and for this reason, the comparison of the two paths had an exploratory rather than a confirmatory character.

As far as the final theoretical question is concerned, I predicted the two types of contact to yield their effects via both ingroup norms and intergroup anxiety (Hypothesis 4). However, I expected the indirect path from *direct* friendships to outgroup attitudes via anxiety to be stronger than the indirect path via ingroup norms, and the indirect path from *extended* friendships to outgroup attitudes via ingroup norms to be stronger than the indirect path via intergroup anxiety for extended friendships (Hypothesis 5).

Method

Participants

Participants were 1000 Greek Cypriots (504 males and 496 females) aged 18 to 87 ($M = 49.81$ years, $SD = 15.09$) who consented to take part in the study. All of the participants were residing at the time of the study in one of the five districts that are currently under the control of the Republic of Cyprus: a) Nicosia ($N = 400$), b) Limassol ($N = 280$), c) Larnaca ($N = 170$), d) Paphos ($N = 100$), and e) Famagusta ($N = 50$). The sampling distribution reflected the distribution of the population of voters for each district.

At Time 1, data were collected from all 1000 participants. At Time 2, there was a 13.9% attrition ($N_{\text{Time2}} = 861$), and at Time 3 there was a further 19.9% attrition ($N_{\text{Time3}} = 690$). The attrition percentage in comparison to the original sample size was 31%.

Procedure

The data were collected via face-to-face interviews based on a structured questionnaire. The interviews were conducted by trained interviewers and took place at the home of the respondents. The Time 1 measures were collected in March 2010, the Time 2 measures between September and October 2010, and the Time 3 measures in April 2011. There was a time-lag of approximately six months between each time point.

Measures

Where possible, constructs were measured with more than one item so that latent variables could be used with Structural Equation Modeling. There are at least two main benefits with using latent variables over observed or aggregated variables especially in longitudinal designs. The first benefit has to do with the measurement error. When observed variables are used, the underlying assumption is that there is no error in the measurement of

the constructs under study (Finkel, 1995). This is an assumption that rarely holds in the social sciences (Christ & Wagner, 2013). In latent variables the error is included in the measurement model and is thus controlled for. Subsequently, the structural model (where relationships between latent variables are specified) can be estimated free of measurement error (Kline, 2010).

The second benefit of using latent variables is that they enable the testing of measurement invariance over time (see Judd & Kenny, 2010; Vandenberg & Lance 2000). According to Vandenberg and Lance (2000), for any meaningful comparisons over time to take place, one needs to establish that the constructs used in the study can be measured by the same set of items across all time points. In essence, one should ensure that the respondents conceptually understand the items they respond to at time *t* in the same way as they do at subsequent time points. If so, then the measures have longitudinal validity, i.e., they are invariant over time (Vandenberg, 2002). Absence of the measurement invariance evaluation, according to Brown (2006), prevents the researcher from being certain that a temporal change in a construct or the relationships between constructs was caused by a “true change or a change in the structure or measurement of the construct over time” (p. 252), and, instead, could arise from the way questions are understood/ answered at different time points.

The questionnaires were identical for all time points and they included the measures for the predictor variables, the mediator variables, and the criterion variable as well as items on the socio-demographic characteristics of the respondents. All of these items were part of a larger questionnaire containing questions on a number of constructs like identification with ingroup, outgroup stereotypes, outgroup trust, perspective taking, and collective guilt. Also, socio-demographic information was available for each participant (e.g., socio-economic

status, political orientation, religiosity, and marital status). I used only those information which I deemed essential and only the items related to the constructs I was interested in.

Socio-demographic characteristics

Participants were asked to provide information about their age, gender, and district of origin.

Predictor variables

Direct friendships were measured by asking participants how many Turkish Cypriot friends they have (1, *none*; 2, *one-two*, 3, *three-five*, 4, *six-ten*, 5, *more than ten*).

Extended friendships were measured by asking participants to think about their Greek Cypriot friends and report how many of them have friends who are Turkish Cypriots (1, *none*; 2, *a few*, 3, *about half*, 4, *more than half*, 5, *most*).

Mediator variables

Intergroup anxiety was measured with a six-item scale which was adapted from Stephan and Stephan's (1985) original intergroup anxiety scale. The instructions asked participants to rate the extent to which they would feel 'anxious', 'nervous', 'awkward', 'safe' (reverse-coded), 'comfortable' (reverse-coded), 'suspicious', if they were the only Greek Cypriot interacting with a group of Turkish Cypriots (1, *not at all*, to 5, *very much*).

Perceived ingroup norms were measured with two items. The participants had to rate the following statements: 1) 'How important is it for your family and friends to be friendly towards Turkish Cypriots?' and 2) 'How important is it for your family and friends to have Turkish Cypriot friends?' (1, *not important at all*; 5, *very important*).

Criterion Variable

Outgroup attitudes were assessed using a scale adapted from Wright et al. (1997) which asked participants to rate their feelings towards Turkish Cypriots on the four following

semantic differential scales: 1, *negative*; 5, *positive*; 1, *cold*; 5, *warm*; 1, *hostile*; 5, *friendly*; 1, *contempt*, 5, *respect*.

Results

Preliminary data analyses

Prior to the main set of analyses I ran preliminary analyses in order to: a) assess the effects of attrition and determine the way to treat missing data, b) assess whether there is nestedness in the data caused by the grouping of the participants in five geographical districts, c) check the normality of each item at each time point with reference to the items' skewness and kurtosis, d) specify the measurement model and e) test for time (measurement) invariance.

Handling of missing data and assessing attrition

Properly dealing with missing data is important since using an inappropriate data handling technique can result in either biased sampling or in attaining biased estimates. One would be in the best place to deal with missing data if one knew the causes of the attrition or rather the nature of the missingness. According to Rubin's typology of missing data (Little & Rubin, 1987; Rubin, 1976), there are three types of missingness depending on what causes the missingness. The first type, Missing Completely at Random, refers to those instances in which the data are missing by a process that happens randomly, i.e., an exogenous variable that is not related to the other variables involved in the model. The second type of missingness, Missing at Random, assumes that the data loss is in fact not random but that the variable that is causing the missingness is known, measured, and it can therefore be controlled for. The third type, Missing not at Random, describes the cases in which the variable causing the missing data is either missing, or is not measured, and cannot therefore be controlled.

Knowing the type of missingness can be of use when determining which data treatment technique can be employed. However, in the vast majority of cases we are not well placed to know the type of missingness. What can be done to some extent is to try to understand why attrition is occurring and more specifically whether there is a systematic reason leading participants to drop out⁶⁶.

As recommended by Goodman and Blum (1996), checking for selective attrition in longitudinal studies is a necessary step to take as it can inform the researcher on whether attrition results in random sampling or not. Attrition is normally assessed via a Multivariate Analysis of Variance (MANOVA) in which all the target variables as well as the demographic variables are entered as dependent variables, and missingness (i.e., whether people dropped out in subsequent waves or not) is the predictor. According to Goodman and Blum (1996) if the analysis indicates that missingness does not predict any of the dependent variables then this would be an indication that the data are missing at random, at least with respect to the variables of interest.

As I had three waves of measurement I performed two MANOVAs. In the first one, the predictor was Time 2 missingness (i.e., individuals who dropped out at Time 2 vs. individuals who completed all three measures) and the dependent variables were all of the target variables plus the demographic characteristics (district, gender, and age) at Time 1. In the second MANOVA, Time 3 missingness (i.e., people who dropped out at Time 3 vs. people who completed all three measures) was the predictor variable. The dependent variables were the same as in the first MANOVA.

⁶⁶ It should be noted though that even when one is able to confirm that no variable from those measured in the study explains attrition there may well be a non-measured constant that can explain non-response at later times which one is not aware of. Consequently, researchers normally resort to making assumptions about the type of missingness and based on these assumptions they end up using the data-technique they deem best.

The results of the first MANOVA showed a significant multivariate effect of missingness (individuals who dropped out at Time 2 vs. participants who completed all three measures) on the dependent variables, $F(8, 615) = 3.80, p < .001$, partial $\eta^2 = .05$. A look at the univariate statistics revealed only one significant difference for age, $F(1, 622) = 23.22, p < .001$, partial $\eta^2 = .04$. The mean age of the individuals who dropped out at Time 2 was lower ($M = 44.22, SD = 15.74$) than the mean age of the participants who completed all three measures ($M = 51.66, SD = 14.69$). The results of the second MANOVA yielded a significant multivariate effect for missingness, $F(8, 634) = 2.36, p < .05$, partial $\eta^2 = .03$. A look at the univariate statistics, however, revealed no significant effects on any of the variables⁶⁷.

I proceeded to consider the missing data as missing at random, at least with respect to the target variables, for two reasons: 1) The multivariate significant effect of missingness for the first MANOVA was due to age differences in the two groups. Even though it is interesting that younger people were more likely to drop out at Time 2, age is not one of the variables under study. There would have been a reason for concern if there were significant differences along the main variables (i.e., the predictor, mediator, and outcome variables) but the effects of missingness on them was far from significant, 2) The second MANOVA did not yield significant differences on the individual dependent variables.

The existent missing data treatment techniques can be divided into two broad categories: a) the traditional pairwise and listwise deletion techniques whereby the missing cases are deleted from the sample either in a listwise or a pairwise manner and b) the Maximum Likelihood methods (see Little & Rubin, 1987) which utilise the Maximum Likelihood algorithms. These Maximum Likelihood algorithms use the information available

⁶⁷ I performed a Bonferroni correction to the p-value for the univariate analyses by dividing the 0.05 by the number of the univariate analyses which equaled the number of the dependent variables (i.e., eight). This gave me a new p-value of 0.006.

from the full sample to infer probable parameter estimates taking into account the uncertainty that arises from missing data (Enders, 2001).

There two main differences between these techniques are: a) listwise and pairwise deletion of missing cases, can only yield unbiased estimates when the data are Missing Completely at Random. This assumption of Missing Completely at Random though is rarely met in practice (Muthen, Kaplan, & Hollis, 1987). If data are not missing completely at random, pairwise and listwise deletion of missing cases will yield biased estimates (e.g., Newman, 2003). The Maximum Likelihood methods on the other hand can yield unbiased estimates even when data are Missing at Random, an assumption that is less stringent and therefore more likely to be true (Enders, 2001), b) Listwise and pairwise deletion can lead to the loss of a great deal of data (Judd & Kenny, 2010) which undermines the power of the analysis. On the contrary, the original sample size is retained when Maximum Likelihood methods are used.

In prior longitudinal research on intergroup contact, missing data were predominantly treated using deletion of missing participants (e.g., Brown et al., 2007; Eller & Abrams, 2004). The dangers related to participant deletion have, however, been recognized (Eller et al., 2011) and Maximum Likelihood methods are now increasingly used by researchers in the field. The three main Maximum Likelihood algorithms available in software packages are: the multiple-group approach, the Full Information Maximum Likelihood estimation, and the Expectation-Maximization algorithm (Enders, 2001). Of those, the most often used so far is the multiple imputation method (see Al Ramiah et al., 2013; Eller et al., 2011) which is related to the Expectation-Maximization algorithm. More recently the Full Information Maximum Likelihood method has also been used (Dhont et al., 2011; Swart et al., 2011).

The Full Information Maximum Likelihood method is suggested for use particularly with large patterns of missing data and when latent variables are utilised (e.g., Enders, 2001; Schafer, 1997). I therefore decided to employ the Full Maximum Likelihood Method to treat missingness in my data which allowed me to retain the original sample size for the analyses. One constraint of all maximum likelihood methods is that they assume multivariate normality of the data (Enders, 2001). This means that any deviations from normality would be likely to give biased estimates with Maximum Likelihood estimators. Therefore I assessed the normality of the distributions of my variables.

Normality tests

I assessed the normality of the distributions of each item in each wave of measurement using the values of skewness and kurtosis as indicators of normality. I adopted West, Finch, and Curran's (1995) cut-off criteria for acceptable values for skewness (-2, 2) and kurtosis (-7, 7). The tests of normality in all measurements and for all items indicated skewness and kurtosis values within the acceptable range. The only exception were the friendship items whose skewness and kurtosis values were either bordering or exceeding the acceptable range. Skewness and kurtosis values for direct friendships across time points were as follows: $T1_{\text{skewness}} = 3.41$, $T2_{\text{skewness}} = 3.70$, $T3_{\text{skewness}} = 3.19$; $T1_{\text{kurtosis}} = 12.01$, $T2_{\text{kurtosis}} = 14.27$, $T3_{\text{kurtosis}} = 10.23$. The skewness and kurtosis values across the three time points for *extended friendships* were: $T1_{\text{skewness}} = 2.03$, $T2_{\text{skewness}} = 1.67$, $T3_{\text{skewness}} = 1.85$; $T1_{\text{kurtosis}} = 6.64$, $T2_{\text{kurtosis}} = 6.10$, $T3_{\text{kurtosis}} = 7.47$. Even though not all of the values lay outside the acceptable range for extended friendships the values were still markedly higher than the skewness and kurtosis values of the rest of the items.

In order to account for the non-normality of these two items, I performed log transformations on them. The new skewness and kurtosis values for each item were well within the acceptable range (*direct friendships*: Skewness: maximum = .71, minimum = .43; Kurtosis: maximum = -1.28, minimum = -1.06; *extended friendships*: Skewness: maximum = .87, minimum = .66; Kurtosis: maximum = -.82, minimum = -.23). Consequently, in all subsequent analyses where the Full Maximum Likelihood estimator was utilised, I used the log-transformed values for these two items.

Testing for nestedness

Since participants were recruited from five different districts, there is a possibility that district as a level-two variable explains some of the variance in the data. In other words the likelihood exists that participants within each district are more similar to each other and more dissimilar to the participants of other districts along the variables of interest. In this case, the observations are thought to be non-independent and any analysis performed at the level of the individual will result in biased estimates. Instead, multi-level analyses would have to be employed.

In order to test for this possibility, I calculated the intra-class correlation coefficient for each construct at each time point. The intra-class correlation coefficient is essentially an indicator of how similar the individuals within each group are. The score represents the proportion of the variance in a construct that is accounted for by the higher level factor, in this case, district. Large intra-class correlation coefficients indicate nestedness in the data, whereas small (close to zero) intra-class correlations signify independence of observations in which case the individual can be used as the unit of analysis. This is particularly the case when the number of level two units is small (Bryk & Raudenbush, 1992; Heck & Thomas, 2008).

Table 5.1: Intra-class Correlation Coefficients (ICCs), standard errors (SEs), and *t*-values

Construct	Time	ICC	SEs	<i>t</i> -value [†]
Direct friendships	T1	0.02	0.01	1.44
	T2	0.02	0.01	1.44
	T3	0.01	0.01	1.10
Extended friendships	T1	0.04	0.02	1.73
	T2	0.06	0.03	1.87
	T3	0.06	0.03	1.87
Ingroup norms	T1	0.07	0.04	1.93
	T2	0.20	0.08	2.41
	T3	0.13	0.06	2.17
Intergroup anxiety	T1	0.03	0.02	1.62
	T2	0.07	0.04	1.93
	T3	0.07	0.04	1.93
Outgroup attitudes	T1	0.03	0.02	1.62
	T2	0.02	0.01	1.44
	T3	0.02	0.01	1.44

[†]*p* = .05 corresponds to a *t* of 1.96 or bigger

To check whether the size of the intra-class correlation coefficients was high enough for the data to be considered as nested, I calculated the intra-class correlations, alongside the standard errors on which the significance of the *t*-values was based. I obtained the intra-class correlations with the use of the multilevel analysis in MPlus by inserting the aggregated variables and the observed variables at each time point and the district as level-two variable. I

calculated the standard errors by dividing the intra-class correlation coefficients by the harmonic mean of the district sample sizes. The harmonic mean is used when the units of the level-two variable do not have equal sample sizes. The values of the intra-class correlations, the standard errors, and the *t*-values are given in *Table 5.1*. Even though some of the intra-class correlations were larger than zero, the *t*-values were non-significant thus suggesting that the intra-class correlations were not large enough for the data to be regarded as nested.

Specification of measurement model

A Principal Component Analysis using the Maximum Likelihood estimator was performed on the items of the three multi-item variables (i.e., ingroup norms, intergroup anxiety, and outgroup attitudes so as to ensure that the items loaded on the expected factors. The three constructs were uni-dimensional at all time points as the items expected to load on them did so without loading on any other factors. *Table 5.2*. gives information on the scale reliability for all multi-item variables, and the mean and standard deviation for all variables.

I then proceeded to the specification of the measurement model, using Confirmatory Factor Analysis with latent factors first for each wave individually and then for all three waves simultaneously. For this purpose, the constructs that were measured with more than one item were treated as latent variables measured by their individual items (manifest indicators). In those cases in which the latent variable was measured by more than three manifest indicators, items were parceled (Little, Cunningham, Shahar, & Widaman, 2002). Parceling refers to the practice of putting together into parcels items that belong to a bigger set. Parceling is a recommended practice because it yields a more parsimonious model which is more likely to have a better fit (in comparison to a less parsimonious model) as the number of parameters to be estimated is reduced (MacCallum, Widaman, Zhang, & Hong, 1999). Little et al. (2002)

contend also that parceled models are more likely to yield solutions that are more stable in comparison to solutions from item-level data⁶⁸.

Table 5.2. Scale reliability (Cronbach's α), Mean, and Standard Deviation for each measure at each time point

	Cronbach's alpha (α)		
	Mean (SD)		
	T1	T2	T3
Direct friendships [†]	1.26 (0.74)	1.22 (0.65)	1.25 (0.67)
Extended friendships [†]	1.46 (0.66)	1.42 (0.52)	1.43 (0.52)
Ingroup norms ^{††}	$r = 0.89^{**}$	$r = .89^{**}$	$r = .88^{**}$
	2.54 (1.10)	2.56 (1.06)	2.64(0.96)
Intergroup anxiety	$\alpha = 0.93$	$\alpha = .94$	$\alpha = .94$
	2.93 (1.13)	2.97 (1.07)	2.98 (1.01)
Outgroup attitudes	$\alpha = 0.94$	$\alpha = .93$	$\alpha = .96$
	2.80 (1.07)	2.78 (0.97)	2.89 (0.92)

[†] No Cronbach's α is given for direct and extended friendships as they were single-item variables.

^{††} Pearson product-moment correlation (r) for a two-item measure, $** p < .01$.

As the variables intergroup anxiety and outgroup attitudes were comprised of more than three indicators, I parceled their items into smaller categories using the item-to-construct

⁶⁸ The debate around parceling, its pros and cons, and the correctness of its use is large and cannot be covered in full here. Most methodologists would tend to agree, however, that especially when: a) the interest is in structural relationships between variables (as opposed to wanting to understand the relationships among items) and b) the factors are uni-dimensional (Bandalos & Finney, 2001), parceling can, or for some should (e.g., Little et al., 2002), be performed.

method of Little et al. (2002). The item-to-construct method suggests that the items are grouped based on their factor loadings: the item with the best factor loading is parceled with the item with the worst factor loading, the item with the second highest loading is parceled with the item with the second worst loading and so on. I therefore grouped the six indicators of intergroup anxiety into three parcels of two indicators each according to their factor loadings. For the variable of outgroup attitudes, which was measured by four indicators, I created one parcel made up by the indicator with the best factor loading and the indicator with the worst factor loading and kept the other two items as manifest indicators⁶⁹.

The measurement model consisted of three latent variables: ingroup norms (measured by two items), intergroup anxiety (measured by three items), and outgroup attitudes (also measured by three items). A wave-by-wave analysis indicated a similar model fit across waves (Wave 1: (N = 1000), $\chi^2(17) = 64.88$, $p < .01$, $\chi^2/df = 3.81$; comparative fit index (CFI) = .99, root-mean-square error of approximation (RMSEA) = .053, standardized root-mean-square residual (SRMR) = .022; Wave 2: (N = 861), $\chi^2(17) = 55.67$, $p < .01$, $\chi^2/df = 3.27$; CFI = .99, RMSEA = .051, SRMR = .027; Wave 3: (N = 690), $\chi^2(17) = 38.77$, $p < .01$, $\chi^2/df = 2.25$; CFI = .99, RMSEA = .043, SRMR = .022). The fit for the measurement model combining all three time points was: (N = 1000), $\chi^2(192) = 324.91$, $p < .01$, $\chi^2/df = 1.69$; CFI = .99, RMSEA = .026, SRMR = .022. The fit indices suggest an acceptable model fit for all models⁷⁰ thus

⁶⁹ This was done because typically a solution with three indicators is better (more stable) in comparison to a solution with two indicators (Little et al., 2002). The fit of the adopted model which consisted of one parcel and two manifest indicators, was compared to the fit of a two-parcel model, and to the fit of the non-parceled model. The adopted model had overall a better fit in comparison to the other two models. The structural relationships between the variables, however, were identical across all these measurement models.

⁷⁰ According to Hu and Bentler (1999) the criteria for an acceptable model fit are a CFI close to .95, a RMSEA close to .06, and a SRMR close to .08. While for a good model fit the χ^2 is expected to be non-significant this is seldom the case (Jöreskog & Sörbom, 1984). Kline (1998) suggested that a chi square: df ratio that is smaller than 3:1 indicates an acceptable model fit but as chi square is extremely sensitive to sample size, the rest of the fit indices should be consulted first (Cheung & Rensvold, 2002).

supporting the proposed factorial structure of the latent constructs at each time point individually as well as for the longitudinal model as a whole.

Test of measurement invariance

There are different types of measurement invariance since there are different sources of invariance. The first type is configural invariance whereby the same construct underlies the same measure at each measurement occasion. This means essentially that the items expected to load on a factor do so consistently over time. The second and more demanding type is called metric invariance and this occurs when both the factor structures (i.e., configural invariance) as well as the loadings of the items on each factor remain unchanged in time. The third type of invariance is scalar invariance which is when the indicators have the same loadings as well as intercepts over time, and the fourth type is residual invariance which is when the indicators have the same loadings, intercepts, and residuals over time (Brown, 2006). The most common practice in contemporary longitudinal research is to demonstrate at least partial measurement invariance (metric invariance) which is often referred to as ‘weak’ measurement invariance (Brown, 2006). Metric invariance is deemed to be adequate for the purposes of a longitudinal research (Ployhart & Vandenberg, 2010) and is set as the minimum criterion for measurement invariance (e.g., Swart et al., 2011).

In order to establish (partial) measurement invariance the full longitudinal measurement model as specified earlier was compared to a more restrictive model in which the loadings of each factor (which were allowed to be freely estimated before) were constrained to equality across time points. The fit of the constrained model was: ($N = 1000$), $\chi^2(200) = 330.28$, $p < .01$, $\chi^2/df = 1.60$; CFI = .99, RMSEA = .026, SRMR = .022. As the constrained and the unconstrained models are nested within each other and therefore differ in

degrees of freedom, the corrected chi-square difference test (Satorra & Bentler, 2001) could indicate whether the constrained model (which is nested within the unconstrained model) has a worse fit in comparison to the unconstrained model or not. A significantly worse fit of the constrained model in comparison to the unconstrained model, indicates that the loadings are not equal across time points and that there is no measurement invariance. The corrected chi-square difference test (Satorra & Bentler, 2001) showed that the model fit of the constrained model was not significantly worse than the fit of the unconstrained model $\Delta\chi^2(8) = 3,81, p = 0.91$, thus confirming partial measurement invariance of the measurement model.

Main analyses

The acceptable model fit of the measurement model along with the establishment of measurement invariance across waves of measurement, allowed me to proceed with the structural model and the investigation of the longitudinal relationships between the variables of interest. Having the invariant measurement model as my base, I advanced to test a set of longitudinal models that corresponded to competing theoretical hypotheses. I initially assessed the model fit of three models: the autoregressive model, the unidirectional ‘forward’ cross-lagged model, and the unidirectional ‘reverse’ cross-lagged model. I then created a bidirectional model and assessed its model fit against the unidirectional models. Lastly, I added to the best fitting model the direct cross-lagged paths between cross-group friendships and attitudes from Time 1 to Time 3 to test for a full versus partial mediation before testing the significance of the indirect effects.

The autoregressive model is a model consisting of only the autoregressive paths for each variable. The underlying hypothesis for this model is that the previous levels of each variable are the only predictors of later levels of the same variable. The unidirectional

‘forward’ model consists of both the autoregressive as well as the ‘forward’ cross-lagged paths, from friendships to mediators and from mediators to attitudes (i.e., Time 1 and Time 2 friendships leading to Time 2 and Time 3 mediators respectively, and Time 1 and Time 2 mediators leading to Time 2 and Time 3 attitudes respectively). The hypothesis underlying this model is that the levels of a variable Y (e.g., attitudes) at time $t + 1$ can be predicted by both the levels of the same variable Y at time t (autoregressive path) and the levels of a second variable X (e.g., friendships) at time t (cross-lagged path). The third model, the unidirectional ‘reverse’ model is identical to the unidirectional ‘forward’ model with the only difference being that the cross-lagged paths have the opposite direction (i.e., Time 1 and Time 2 attitudes predict Time 2 and Time 3 mediators, and Time 1 and Time 2 mediators predict Time 2 and Time 3 friendships respectively). The ‘forward’ cross-lagged model corresponds to the hypothesis that friendships have a temporal effect on attitudes (via the mediators) whereas the ‘reverse’ cross-lagged model would test the hypothesis that attitudes predict friendships (via the mediators).

The key element about these models is that they are nested in relation to each other and they therefore differ in degrees of freedom. More specifically, the autoregressive model which has the highest number of degrees of freedom is nested within the two unidirectional models. Given the differences in degrees of freedom the alternative models could be compared to each other in terms of their relative fit to the data using the corrected chi-square difference test (Satorra & Bentler, 2001). When comparing models that are nested within each other, the chi-square difference test, can inform us whether the nested model has a significantly worse fit in comparison to the parent model (i.e., the model in which it is nested). More specifically, the comparison of the autoregressive model (nested model) to the

unidirectional models (parent models) can tell us whether the autoregressive solution has a significantly poorer fit to the data in comparison to the parent models. If so, then that would mean that the cross-lagged paths are necessary in order to explain significantly more variance in the data.

Furthermore, each of the aforementioned models has an unconstrained version where the paths from Time 1 to Time 2 and from Time 2 to Time 3 are freely estimated, and a constrained (or stationary) version where all paths from Time 1 to Time 2 and from Time 2 to Time 3 are constrained to equality⁷¹. Once again, as the constrained and the unconstrained models differ in degrees of freedom (the constrained model is nested within the unconstrained model), their fit can be compared using the corrected chi-square difference test (Satorra & Bentler, 2001). The constrained model is only adopted if it does not lead to a significantly worse fit in comparison to the unconstrained model.

I followed the model-comparison strategy of Swart et al. (2011) in order to identify the model that fit my data best. *Table 5.3.* gives the fit indices of each model tested, and the results of the model comparison. I started off by comparing the unconstrained autoregressive model (Model 1a) for which the Time 1-Time 2, Time 2-Time 3 autoregressive paths were freely estimated to the stationary autoregressive model (Model 1b) where the paths from Time 1 to Time 2 were constrained to be equal to the paths from Time 2 to Time 3. This comparison revealed that the constrained model did not lead to a worse fit in comparison to the unconstrained model, which meant that that the autoregressive paths could be regarded as stationary.

⁷¹ As highlighted in the introduction to this chapter one of the assumptions that needs to hold for mediation estimates to be regarded as unbiased is the stationarity assumption. Even though only path equivalence across time for the autoregressive paths is required for the stationarity condition to be met (see Cole & Maxwell, 2003) for purposes of model parsimony I also attempted to constrain the cross-lagged paths to equality (see Swart et al., 2011).

Building on the most parsimonious autoregressive model (i.e., Model 1b), I proceeded to check the fit of the ‘forward’ cross-lagged model by adding the paths from Time 1/ Time 2 friendships to Time 2/ Time 3 mediators and the paths from Time 1/ Time 2 mediators to Time 2 / Time 3 attitudes respectively. For the unconstrained version of the ‘forward’ cross-lagged model (Model 2a), I allowed the aforementioned paths to be freely estimated, whereas for the constrained version of this model (Model 2b) I constrained the cross-lagged paths from Time 1 to Time 2 to be equal to their corresponding Time 2 to Time 3 paths. The constrained ‘forward’ cross-lagged model (Model 2b) did not have a worse fit in comparison to the unconstrained model (Model 2a) which allowed me to adopt Model 2b and compare it to the most parsimonious autoregressive model (Model 1b). This comparison yielded a significantly worse fit of the autoregressive model in comparison to the cross-lagged model thus indicating that (at least some of) the ‘forward’ cross-lagged paths were significant.

The exact same process was followed to test whether the constrained ‘reverse’ model (Model 3b) had a worse fit or not in comparison to its unconstrained version (Model 3a) and to compare the most parsimonious ‘reverse’ model to the most stationary autoregressive model. The comparisons revealed that the constrained model (Model 3a) did not have a significantly poorer fit in comparison to the unconstrained model (Model 3b) and that the autoregressive model (Model 1b) had a significantly worse fit in comparison to the most parsimonious ‘reverse’ cross-lagged model (Model 3b). The last comparison suggested that (at least some of) the ‘reverse’ paths were significant and had to be included for the model to explain significantly more variance.

Since I expected that both the ‘forward’ and the ‘reverse’ paths would simultaneously be in effect (Hypotheses 1 and 3) and given that the fit indices for the two unidirectional

models did not seem to differ considerably⁷², I proceeded to merge the two into a bidirectional model (Model 4) in order to test whether a model comprised of both the ‘forward’ and the ‘reverse’ cross-lagged paths fit the data better than each of the unidirectional models. Model 4, consisting of the constrained versions of the unidirectional models, did fit the data better than both the unidirectional models.

Lastly, as I did not a priori assume full mediation, i.e., that the two mediators would fully mediate the effect of friendships on attitudes, I tested three more models where I added first the ‘forward’ direct paths from Time 1 friendships to Time 3 attitudes (Model 6a), then the ‘reverse’ direct paths from Time 1 attitudes to Time 3 friendships (Model 6b) and then both the ‘forward’ and the reverse paths direct paths between Time 1 friendships/attitudes to Time 3 friendships/attitudes (Model 6c) to the best fitting model (Model 4). This allowed me to test for a full versus partial mediation.

However, in order to include the cross-lagged paths from Time 1 to Time 3, I had to first add to Model 4 the corresponding autoregressive paths, i.e., the path from Time 1 direct friendships to Time 3 direct friendships, the path from Time 1 extended friendships to Time 3 extended friendships, and the path from Time 1 attitudes to Time 3 attitudes. Consequently, Model 5a consisted of the paths in Model 4 plus the autoregressive path from Time 1 attitudes to Time 3 attitudes and served as a comparison to Model 6a (i.e., the model incorporating the ‘forward’ cross-lagged paths from Time 1 direct and extended friendships to Time 3 outgroup attitudes to Model 5a). Similarly, Model 5b consisted of the all the paths of Model 4 plus the autoregressive paths from Time 1 to Time 3 direct and extended friendships. Model 5b served as a comparison to Model 6b (i.e., the model incorporating the ‘reverse’ cross-lagged paths

⁷² No chi-square difference test could be performed for the two unidirectional cross-lagged models as they were not nested in each other. A look at the fit indices of the two models suggests that the ‘forward’ model fits the data marginally better than the ‘reverse’ model.

from Time 1 attitudes to Time 3 direct and extended friendships to Model 5b). Lastly, Model 5c was a combination of Model 5a and Model 5b and so it included the autoregressive paths from Time 1 extended/direct friendships and outgroup attitudes to Time 3 extended/ direct friendships and outgroup attitudes respectively. Model 5c served as a comparison to Model 6c (i.e., the model incorporating both the ‘forward’ cross lagged paths from Time 1 direct/extended friendships to Time 3 outgroup attitudes and the corresponding ‘reverse’ paths to Model 5c).

Of the models including both the Time 1-Time 3 autoregressive and cross-lagged paths (Models 6a-c), none was a better fit than their corresponding models containing only the autoregressive paths (Models 5a-c). This finding essentially suggested that none of the cross-lagged direct paths from Time 1 direct/extended friendships to Time 3 outgroup attitudes and the ‘reverse’ cross-lagged paths from Time 1 outgroup attitudes to Time 3 were significant. Of Models 5a-c only Model 5a and Model 5c had a significantly better fit in comparison to Model 4, and similarly of Models 6a-c, only Model 6a and Model 6c had a significantly better fit than Model 4. Furthermore there was no difference in fit between Model 6a (the model including the ‘forward’ cross-lagged paths from Time 1 friendships to Time 3 outgroup attitudes and the corresponding autoregressive paths) and Model 6c (the model including both ‘forward’ and ‘reverse’ autoregressive paths) where Model 6c had a significantly better fit than Model 6a. In a nutshell, what these model comparisons suggest is that the neither the ‘forward’ nor the ‘reverse’ cross-lagged paths from Time 1 to Time 3 had any contribution to make over-and-above the Time 1-Time 3 autoregressive paths, and that of the autoregressive paths only the path from Time 1 outgroup attitudes to Time 3 outgroup attitudes was significant. Indeed, a look at the Model 6c which will be presented in detail next revealed that

the ‘forward’ paths from Time 1 direct and extended friendships to Time 3 attitudes and the ‘reverse’ direct cross-lagged paths from Time 1 attitudes to Time 3 friendships (direct and extended) were all non-significant.

Since, however, Model 6c was the model that tested the full ‘forward’ and ‘reverse’ mediation hypothesis which was of interest, and since this model (Model 6c) was significantly better than Model 4, I ended up adopting it. Model 6c is presented next in detail. All significant paths are outlined in *Figure 5.1*. In the text following I will present both the significant and the non-significant paths.

Bidirectional longitudinal model with direct (Time 1-Time 3) ‘forward’ and ‘reverse’ cross-lagged paths between cross-group friendships and outgroup attitudes (Model 6c):

As seen in *Figure 5.1*, all the autoregressive as well as all the cross-lagged paths from Time 1 to Time 2 are equal to their corresponding Time 2 to Time 3 paths. This is because the bidirectional model was comprised of the stationary autoregressive model as well as the stationary unidirectional models for which the Time 1 to Time 2 and the Time 2 to Time 3 paths were constrained to equality.

In line with Hypothesis 1 both direct and extended friendships at Time 1 had an indirect effect on outgroup attitudes at Time 3. More specifically, direct friendships at Time 1 had an effect on both intergroup anxiety and ingroup norms at Time 2. Direct friendships at Time 1 were negatively associated with intergroup anxiety ($b = -.55, p < .01, 99\% \text{ CI } [-.96, -.14]$)⁷³, and positively associated with ingroup norms ($b = .44, p < .05, 95\% \text{ CI } [.09, .79]$) at Time 2. Extended friendships at Time 1 had no significant effect on Time 2 ingroup norms (b

⁷³ The beta coefficients for the effects of direct and extended friendships are seemingly very large. This is because these two variables were log-transformed for in order to normalize them. The corresponding standardized coefficients for the betas reported here are: for the path from direct friendships to intergroup anxiety: $b = -0.07, p < .01$; for the path from direct friendships to ingroup norms: $b = .06, p < .05$; for the path from extended friendships to intergroup anxiety: $b = -.05, p < .05$.

= .09, $p = \text{ns}$, 95% CI [-.23, .40]) but they had an effect on intergroup anxiety at Time 2. Extended friendships at Time 1 were negatively associated with intergroup anxiety at Time 2 ($b = -.37$, $p < .05$, 95% CI [-.69, -.03]). Intergroup anxiety at Time 2 was negatively associated with outgroup attitudes at Time 3 ($b = -.07$, $p < .01$, 99% CI [-.13, -.01]) whereas ingroup norms at Time 2 were positively associated with outgroup attitudes at Time 3 ($b = .11$, $p < .001$, 95% CI [.04, .18]). Furthermore, there were no direct effects from Time 1 direct friendships to Time 3 outgroup attitudes ($b = .17$, $p = \text{ns}$, 95% CI [-.29, .60]) or from Time 1 extended friendships to Time 3 outgroup attitudes ($b = .11$, $p = \text{ns}$, 95% CI [-.33, .53]). The absence of direct paths from Time 1 direct and extended friendships to Time 3 outgroup attitudes indicated full mediation of the effects of both types of friendships on attitudes.

In order to check the significance of the indirect effects I ran mediation analyses using bootstrapping. Bootstrapping is particularly recommended when there are asymmetries or other forms of non-normality in the sampling distribution (see Preacher & Hayes, 2004). Three indirect effects were tested: 1) the indirect effect of Time 1 direct friendships on Time 3 outgroup attitudes via intergroup anxiety, 2) the indirect effect of Time 1 direct friendships on Time 3 outgroup attitudes via Time 2 ingroup norms, and 3) the indirect effect of Time 1 extended friendships on Time 3 outgroup attitudes via Time 2 intergroup anxiety. Bootstrap analyses revealed that all three indirect effects were significant. The indirect effect of Time 1 direct friendships on Time 3 outgroup attitudes via Time 2 intergroup anxiety was significant at the 0.01 level (point estimate: 0.04 with a 99% confidence interval of [0.01, 0.09]), and the indirect effect of Time 1 direct friendships on Time 3 outgroup attitudes via Time 2 ingroup norms was significant at the 0.01 level (point estimate: 0.05 with a 99% confidence interval of [0.002, 0.12]). The indirect effect of extended friendships at Time 1 on outgroup attitudes at

Time 3 via Time 2 intergroup anxiety was significant at the 0.05 level (point estimate: 0.03 with a 95% confidence interval of [0.003, 0.06]).

Hypothesis 2 predicted that if there were any differences between the effects of the two friendship types there would be a stronger effect for direct friendships. As the direct paths from Time 1 friendships to Time 3 outgroup attitudes were not significant, to test Hypothesis 2, I compared the indirect effects of each type of friendship on outgroup attitudes⁷⁴. The sum of the indirect effect of Time 1 friendships on Time 3 attitudes via the two mediators was significant at the 0.01 level (point estimate: 0.09 with a 99% confidence interval of [0.02, 0.18]). I did not need to sum the indirect effects of Time 1 extended friendships on Time 3 attitudes as the relationship between extended friendships and attitudes was only mediated by one mediator, intergroup anxiety. This comparison yielded a significant difference between the overall indirect effect of Time 1 direct friendships on Time 3 attitudes and the indirect effect of Time 1 extended friendships on Time 3 outgroup attitudes ($z = -2.96, p < .01$) where the indirect effect of direct friendships on attitudes was, as Hypothesis 2 predicted, larger than the corresponding effect for extended friendships.

Hypothesis 3 contented that the ‘reverse’ longitudinal mediation from Time 1 outgroup attitudes to Time 3 friendships would be significant. According to Model 6c, the only ‘reverse’ paths that were significant were the paths from outgroup attitudes at Time 1 to Time 2 ingroup norms and intergroup anxiety. More specifically, outgroup attitudes at Time 1 were negatively associated with intergroup anxiety ($b = -.08, p < .01, 99\% \text{ CI } [-.15, -.01]$) and positively associated with ingroup norms ($b = .19, p < .01, 99\% \text{ CI } [.11, .27]$) at Time 2.

Intergroup anxiety at Time 2, however, did not have an effect on direct ($b = -.001, p = \text{ns}, 95\%$

⁷⁴ I compared the two indirect effects based on their unstandardized betas and standard errors while also taking sample size into account (see Goodman & Blum, 1996). I derived the unstandardized coefficients and standard errors of the indirect paths using the Sobel test for mediation.

CI [-.01, .005]) or extended ($b = -.01, p = \text{ns}, 95\% \text{ CI } [-.01, .004]$) friendships at Time 3.

Ingroup norms at Time 2 did not have an effect on direct ($b = -.001, p = \text{ns}, 95\% \text{ CI } [-.01, .01]$) or extended ($b = .00, p = \text{ns}, 95\% \text{ CI } [-.01, .01]$) friendships at Time 3 either.

Furthermore, neither the ‘reverse’ direct cross-lagged path from outgroup attitudes at Time 1 to direct friendships at Time 3 ($b = -.004, p = \text{ns}, 95\% \text{ CI } [-.02, .01]$) nor the ‘reverse’ cross-lagged path from Time 1 outgroup attitudes to Time 3 extended friendships ($b = .01, p = \text{ns}, 95\% \text{ CI } [-.01, .02]$) was significant. These findings did not support my hypothesis for the existence of a ‘reverse’ (direct or indirect) path from attitudes to cross-group friendships.

I expected that the effect of Time 1 cross-group friendships on Time 3 attitudes would be mediated by both intergroup anxiety and ingroup norms, for direct as well as extended friendships (Hypothesis 4). The results did not support this hypothesis. While the effects of Time 1 direct friendships on Time 3 outgroup attitudes were explained by both mediating variables, intergroup anxiety and ingroup norms, only intergroup anxiety mediated the effects of Time 1 extended friendships on Time 3 outgroup attitudes. Hypothesis 5 could be tested only for direct friendships for which both mediators were in effect. The comparison of the two indirect effects yielded no significant differences ($z = -.50, p = \text{ns}$). Intergroup anxiety and ingroup norms did not differ in terms of how strongly they mediated direct friendships effects on attitudes. Interestingly, although Hypothesis 5 predicted that ingroup norms would be the strongest out of the two mediators for extended friendships, norms did not even mediate the effects of Time 1 extended friendships on Time 3 outgroup attitudes.

Table 5.3: Model fit and comparisons of autoregressive, unidirectional, and bidirectional longitudinal models

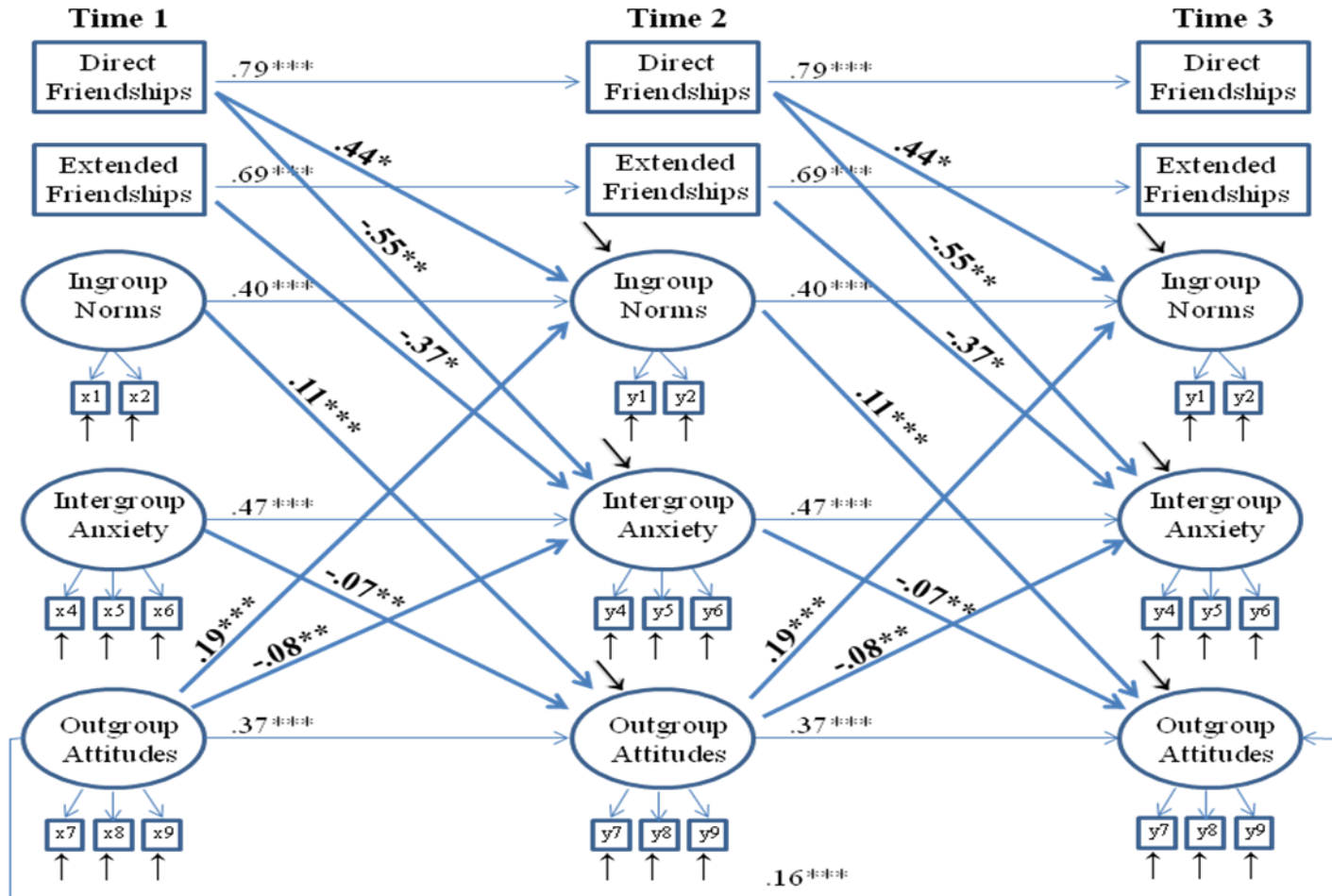
Model	Model fit	Models Compared	Corrected χ^2 difference (<i>df</i>)
1a: autoregressive model (freely estimated parameters)	$\chi^2(349) = 840.91^{***}$, CFI = .98, RMSEA = 0.038, SRMR = .13		
1b: autoregressive model (within construct path equivalence)	$\chi^2(354) = 844.83^{***}$, CFI = .98, RMSEA = 0.037, SRMR = .13	1b vs. 1a	$\Delta\chi^2(5) = 5.27, p = .38$
2a: unidirectional "forward" model: friendships → mediators → attitudes (freely estimated parameters)	$\chi^2(342) = 742.05^{***}$, CFI = .98, RMSEA = 0.034, SRMR = .08		
2b: unidirectional "forward" model (cross-lagged path equivalence),	$\chi^2(348) = 746.30^{***}$, CFI = .98, RMSEA = 0.034, SRMR = .08	2b vs. 2a 2b vs. 1b	$\Delta\chi^2(6) = 3.19, p = .78$ $\Delta\chi^2(5) = 109.12^{***}$
3a: unidirectional "reverse" model: attitudes → mediators → friendships (freely estimated parameters)	$\chi^2(342) = 755.44^{***}$, CFI = .98, RMSEA = 0.035, SRMR = .09		
3b: unidirectional "reverse" model (cross-lagged path equivalence)	$\chi^2(348) = 764.31^{***}$, CFI = .98, RMSEA = 0.035, SRMR = .09	3b vs. 3a 3b vs. 1b	$\Delta\chi^2(6) = 8.87, p = .18$ $\Delta\chi^2(5) = 80.52^{***}$
4: bidirectional model (Model 2b + Model 3b)	$\chi^2(342) = 698.74^{***}$, CFI = .98, RMSEA = 0.032, SRMR = .07	4 vs. 1b 4 vs. 2b 4 vs. 3b	$\Delta\chi^2(6) = 153.04^{***}$ $\Delta\chi^2(6) = 47.56^{***}$ $\Delta\chi^2(6) = 72.04^{***}$
5a: Model 4a + Time 1 – Time 3 autoregressive path for outgroup attitudes	$\chi^2(341) = 668.75^{***}$, CFI = .98, RMSEA = 0.031, SRMR = .06	5a vs. 4	$\Delta\chi^2(1) = 29.99^{***}$
5b: Model 4a + Time 1 – Time 3 autoregressive paths for direct and extended friendships	$\chi^2(340) = 698.90^{***}$, CFI = .98, RMSEA = 0.032, SRMR = .07	5b vs. 4	$\Delta\chi^2(1) = \text{ns}$
5c: Model 5a + Model 5b	$\chi^2(339) = 667.68^{***}$, CFI = .98, RMSEA = 0.031, SRMR = .06	5c vs. 4	$\Delta\chi^2(3) = 24.95^{***}$
6a: Model 5a + Time 1- Time 3 'forward' cross-lagged paths from direct/extended friendships to attitudes	$\chi^2(339) = 667.84^{***}$, CFI = .98, RMSEA = 0.031, SRMR = .06	6a vs. 5a	$\Delta\chi^2(2) = .91, p = .63$

Table 5.3 continued

6b: Model 5b + Time 1- Time 3 'reverse' cross-lagged paths from attitudes to direct/extended friendships	$\chi^2(338) = 697.21^{***}$, CFI = .98, RMSEA = 0.033, SRMR = .07	6b vs. 5b	$\Delta\chi^2(2) = 1.69, p = .43$
		6b vs. 4	$\Delta\chi^2(4) = 2.83, p = .59$
6c: Model 6a + Model 6b	$\chi^2(335) = 664.88^{***}$, CFI = .98, RMSEA = 0.031, SRMR = .06	6c vs. 5c	$\Delta\chi^2(4) = 2.80, p = .59$
		6c vs. 6a	$\Delta\chi^2(4) = 3.91, p = .42$
		6c vs. 6b	$\Delta\chi^2(3) = 32.33^{***}$
		6c vs. 4	$\Delta\chi^2(7) = 31.47^{***}$

CFI = Comparative Fit Index; RMSEA = Root-Mean-Square Error of Approximation; SRMR = Standardized Root-Mean-Square Residual

Figure 5.1. A bidirectional model showing the indirect effects of Time 1 direct and extended friendships on Time 3 outgroup attitudes, mediated by ingroup norms (for direct friendships) and intergroup anxiety (for both direct and extended friendships) at Time 2 (Model 6c in Table 5.3.)



Note: Only significant paths are presented (* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$); all coefficients reported are unstandardized. Autoregressive and cross-lagged paths from Time 1 to Time 2 and their corresponding paths from Time 2 to Time 3 are constrained to equality. Cross-lagged paths are in bold.

Discussion

The present study aimed to advance knowledge with regards to the longitudinal relationship between cross-group friendships and prejudice by addressing three new theoretical questions. The first question pertained to whether extended, like direct, friendships have a longitudinal effect on prejudice (outgroup attitudes) in a low-intergroup contact context and, if so, whether this effect is comparable to the longitudinal effect of direct friendships on prejudice. The second question addressed the selection bias issue, i.e., whether individuals with more positive attitudes end up having more friendships (direct or extended) in the long run. The third question investigated whether the longitudinal effects of direct and extended friendships were mediated by the same or different variables. I will summarize the main findings of this study before discussing the aforementioned theoretical questions. Finally, I will discuss some limitations of the study and offer suggestions for future research.

Five hypotheses were derived for the three research questions:

Hypothesis 1: both direct and extended friendships at Time 1 were expected to predict (directly or indirectly) more positive attitudes at Time 3.

Hypothesis 2: if there were any differences between the two types of friendships in terms of their temporal effects on attitudes, these would show direct friendships to have stronger effects in comparison to extended friendships.

Hypothesis 3: the 'reverse' paths would be significant for both types of friendships; Time 1 outgroup attitudes would have an effect (direct or indirect) on cross-group direct and extended friendships at Time 3.

Hypothesis 4: both proposed mediators, ingroup norms and intergroup anxiety, would mediate the effects of Time 1 direct and extended friendships on Time 3 attitudes.

Hypothesis 5: intergroup anxiety would be the strongest mediator of the effects of direct friendships on attitudes, whereas ingroup norms would be the strongest mediator of the effects of extended friendships on attitudes.

The results were in line with Hypothesis 1; Time 1 friendships (direct and extended) gave rise to more positive attitudes at Time 3. This effect was indirect, Time 1 friendships led to Time 2 mediators which in turn led to Time 3 outgroup attitudes. The direct paths from Time 1 friendships to Time 3 attitudes were not significant for either type of friendships. Furthermore, as predicted by Hypothesis 2, direct friendships had a significantly stronger (indirect) effect on attitudes in comparison to extended friendships. Contrary to Hypothesis 3, there was no effect extending from Time 1 attitudes to Time 3 direct or extended friendships. The only significant 'reverse' paths were the paths from Time 1 attitudes to Time 2 intergroup anxiety and Time 2 ingroup norms, with more positive attitudes at Time 1 predicting less intergroup anxiety and more favourable ingroup norms at Time 2. There were no significant paths, however, going from Time 2 ingroup norms and intergroup anxiety to Time 3 direct or extended friendships, and therefore no support was given to the indirect effect of Time 1 outgroup attitudes on Time 3 friendships via the mediators.

Lastly, with regards to the mediation of the temporal effects of the two types of friendships on outgroup attitudes, the effects of Time 1 direct friendships on Time 3 attitudes were found to be mediated by both proposed mediators, as was expected (Hypothesis 4). For extended friendships, however, its effect on Time 3 attitudes was mediated only by intergroup anxiety. Hypothesis 5 could not be fully tested as it presupposed that the effects of direct as well as extended friendships on Time 3 attitudes would be mediated by both of the proposed mediators. Ingroup norms were expected to emerge as the strongest mediator of the effects of

extended friendships on attitudes. Extended friendships at Time 1, however, were shown not to have a significant effect on Time 2 ingroup norms and as a consequence of that, ingroup norms did not mediate the effects of Time 1 extended friendships on Time 3 outgroup attitudes. Intergroup anxiety was expected to be the strongest of the two mediators mediating the effects of Time 1 direct friendships on Time 3 attitudes. However, the two mediators, intergroup anxiety and ingroup norms, did not differ with respect to how strongly they mediated the longitudinal effects of direct friendships on attitudes.

Are the longitudinal effects of extended friendships comparable to the effects of direct friendships?

Very little has been learned so far about the longitudinal effects of extended friendships. Like in cross-sectional studies, extended contact was found by longitudinal studies to be effective in contexts where opportunities for contact are scarce and where, by extension, individuals have no or very little direct intergroup contact (e.g., Christ et al., 2010)⁷⁵. On the contrary, in non-segregated contexts where individuals reported having a fair amount of direct contact in the form of cross-group friendships, the effects of extended friendships cease to exist (see Feddes et al., 2009).

The intergroup contact reported by the participants of the current study was unsurprisingly low. A context of low contact, as discussed above, is a context where extended friendships are more likely to have effects that are not suppressed by direct friendships, and that is what the current study has indeed shown. What was left unanswered from past studies was how the effects of extended friendships fare against the effects of direct friendships when both effects are present. Past literature from cross-sectional studies (e.g., Turner et al., 2008)

⁷⁵ A recently published study by Munniksma et al. (2013) found that extended friendships had temporal effects on attitudes only amongst those individuals who held less positive outgroup attitudes at Time 1.

has shown superior effects of extended friendships in contexts of low direct contact but otherwise the effects of direct friendships were reported to be more profound than the effects of extended friendships (see e.g., Paolini et al., 2004; 2007). My hypothesis was that the effects of direct friendships would be greater than the effects of extended friendships because of the unique importance of experiencing a high quality first-hand interaction with one or more outgroupers in comparison to merely knowing about it via a friend.

While the results support my hypothesis (direct friendships had a greater longitudinal indirect effect on attitudes than extended friendships did) there are a few points worth considering here. Firstly, comparing the two types of friendships on one dependent variable may not be indicative of the complete picture as direct and extended friendships may simply be tapping different psychological processes (e.g., Pettigrew, et al., 2007) and outcomes (e.g., Paolini et al., 2004) or they may be more or less effective depending on the bases (cognitive or affective) of one's prejudice (Paolini et al., 2007). The studies of Paolini et al. (2004; 2007) suggest overall a stronger effect of direct over extended friendships on affect-related outcomes (e.g., attitudes instead of perceived outgroup variability) and for outgroups which elicit more emotional than cognitive responses. The results of the current study are largely in line with these findings at least with regards to how direct friendships in comparison to extended friendships lead to stronger overall effects for an affect-based outcome (i.e., outgroup attitudes) towards an affective outgroup (i.e., Turkish Cypriots).

Secondly, not much is known about why the effects of extended friendships manifest in segregated contexts where intergroup contact is low. The explanation put forward by Christ et al. (2010) is that the differential effects of extended friendships on prejudice in mixed versus segregated contexts are explained by the difference in the amount of direct friendships

in the two contexts. This proposed explanation (mediator) was given support in the study by Christ et al. (2010) as the amount of direct contact did indeed mediate the differential effects of extended friendships in the two contexts. I am, however, reluctant to accept the lack of direct friendships as the only reason for which the effects of extended friendships manifest primarily in segregated contexts. My alternative or complementary explanation for why contact has different effects in mixed versus segregated contexts, apart from the reason proposed by Christ et al. (2010), is that intergroup contact per se is likely to be perceived and/or experienced differently in each of these contexts.

To be more exact, in contexts where the levels of contact are low, intergroup contact (direct or extended) as an occurrence is more likely to stand out as opposed to mixed contexts where contact is, one could say, expected. Having intergroup contact of any kind in Cyprus, for example, is a prominent event that hardly goes unnoticed, whereas having intergroup contact in an extremely diverse and tolerant setting may well be something that is overlooked or that is perceived to be commonplace. My assertion is that in contexts where intergroup contact is not the norm, direct, as well as extended friendships, will form useful sources (in fact the only sources) of information about an outgroup that is otherwise rarely seen or talked about. As such, both types of friendships may constitute formative events which can (and as results show, do) contribute to one's evaluation of the outgroup. Even though "normative support" has been thought to be a facilitating factor for intergroup contact (Allport, 1954; Dovidio et al., 2003; Pettigrew & Tropp, 2006), my argument here is that when an individual manages against all odds to have positive intergroup experiences directly or indirectly, these experiences are likely to form highlights that would profoundly influence one's evaluation of the outgroup. On the contrary, I believe that in mixed settings intergroup experiences are less

likely to form memorable and influential highlights unless they are as intimate and as tangible as face-to-face experiences with outgroup members. Hence, the effects of direct friendships on attitudes are more profound in these settings.

Are less prejudiced individuals more likely to have direct as well as extended friendships in the long run?

An unexpected finding of this study was the absence of ‘reverse’ paths from attitudes to friendships. I expected selection bias to occur amongst Greek Cypriots since for contact to be achieved in Cyprus individuals have to, in most cases, take the deliberate decision to pursue it. I therefore expected individuals with more positive feelings towards the outgroup to have more direct as well as extended friendships. The results did not support my hypothesis. Even though holding positive attitudes at Time 1 did lead to more favourable ingroup norms and less intergroup anxiety at Time 2, these positive orientations towards the outgroup and outgroup contact did not result in more actual outgroup friendships at Time 3.

A possible explanation of this result may be found in the absence of opportunities for contact in the Cypriot context, where the two communities live, work, and socialize on separate sides of the island. It might be the case that individuals who hold positive attitudes and have less anxiety to meet the outgroup, and whose close ingroupers do in fact support contact, simply do not have the opportunities to have contact which is why their positive outlook of the outgroup doesn’t translate into actual intergroup contact let alone friendships. These individuals would probably be open to having contact if the opportunity emerged. In other words, willingness to pursue contact (as expressed here via favourable attitudes, low anxiety for intergroup contact, and positive ingroup norms) may not be predictive of these

individuals generating more opportunities for contact out of nowhere, but it could instead be predictive of them utilizing opportunities for contact when those are available.

Are there any differences in the mediation of the longitudinal effects of direct and extended friendship?

Direct and extended friendships in this study were found to have longitudinal indirect effects on prejudice (outgroup attitudes). While it was hypothesized, based on previous studies, that the proposed mediators, intergroup anxiety and ingroup norms, would mediate the effects of both direct and extended friendships on attitudes, this turned out not to be the case for extended friendships. Before focusing on the differences in the mediational processes, one important similarity should be underlined. Both types of friendships had a temporal effect on intergroup anxiety. This was the most consistent finding in all the studies of this thesis which I compared direct and extended contact. In Experiments 2 and 3 of Chapter 3, a single instance of intergroup contact did not lead to a more permanent change in outgroup attitudes, but it did, however, lead to a robust effect in reducing anxiety, an effect that did not fade away a week after contact. I suggested then that anxiety reduction may be a first step towards other positive intergroup outcomes. The results of this longitudinal study provide support for this assertion.

In order to further test this hypothesis with the current data, I tested one more model where I switched the order of the mediators with the outcome variable so that attitudes became the mediating variable and intergroup anxiety and ingroup norms became the outcome variables. This model had a significantly worse fit in comparison to Model 4⁷⁶ and, moreover, the paths from Time 1 friendships (either type) to Time 2 outgroup attitudes were not

⁷⁶ I used Model 4 (i.e., the bidirectional model including all the Time 1–Time 2, Time 2–Time 3 paths from friendships to mediators to outgroup attitudes and the ‘reverse’ paths) as I was not interested here in comparing the direct paths (i.e., the paths from Time 1 contact to Time 3 outcome variables and the ‘reverse’ paths) of the two models. The fit of the alternative model was: $\chi^2(346) = 720,72$, CFI = .982, RMSEA = .033, SRMR = .07, $\Delta\chi^2(4) = 23.20$, $p < .001$

significant. This finding shows that the route from intergroup contact to prejudice (at least in this context) goes through intergroup anxiety and that intergroup anxiety, is indeed the first step towards other positive outgroup outcomes.

Less clear-cut were the findings for ingroup norms. Time 1 extended friendships were expected but not found to, lead to a change in Time 2 ingroup norms. This null effect is in disagreement with results of previous (cross-sectional) studies (e.g., Cameron et al., 2011; De Tezanos-Pinto et al., 2010; Gomez et al., 2011). As pointed out already, however, no longitudinal effects of extended friendships on ingroup norms have been reported in the past. Feddes et al. (2009), who included a composite measure of ingroup and outgroup norms as a potential mediating variable explaining the effects of extended (as well as direct) friendships on attitudes, found no effects of extended friendships at Time 1 on Time 2 intergroup norms. If the effects of extended friendships on norms have been demonstrated in cross-sectional but not in longitudinal studies to date then this may indicate that these effects are not strong enough to maintain over time.

A question still pending is why direct and not extended friendships have a temporal effect on ingroup norms. A proposed explanation in line with the arguments of Davies et al. (2013) on why direct friendships should be expected to influence ingroup norms, could be that in a context where the norms for intergroup contact are unclear, one needs to rely on one's own experiences to infer these norms. Extended friendships are presumably less informative in comparison to direct friendships and this is why their effect on ingroup norms is rather superficial and does not manage to solidify. Direct friendships which are much more intimate and provide the individuals with richer information about intergroup relations may in these

instances be more catalytic in forming one's norms in comparison to the less-informative extended friendships.

This explanation may be perceived, however, to be weak especially since the same differences between direct and extended friendships with regards to their effects on norms were found by Feddes et al. (2009) in a context where contact norms did not seem to be that unclear. One could counter-argue here that despite the context being more straightforward in terms of social norms in the study of Feddes et al. (2009) the respondents their study were young adolescents whose normative beliefs are still under formation (see Abrams & Rutland, 2008). As an effect of that, for this group too more intimate and informative experiences such as those attained via direct friendships should probably have a greater bearing on the formation of social norms.

Study limitations and future research

As underlined earlier in the discussion of the results of this study, comparing the two types of friendships only in respect to one outcome variable may not be doing justice especially to extended friendships which were thought and found (see Paolini et al., 2004; 2007) to be more influential in changing more cognitive-based outcomes (e.g., perceived outgroup variability) and to have greater prejudice reduction effects toward more 'cognitive' outgroups. However, the purpose of this study was not to find which outcome variable each type of contact was better able to influence but rather whether extended just like direct friendships could produce more favourable attitudes toward an outgroup with very affective connotations for the participants of this study. I chose outgroup attitudes over other outcome variables as they are the standard, or default, outcome measure in studies of intergroup contact, and because they themselves have cognitive, affective, and behavioural aspects.

Testing alternative outcome variables, however, could have been of interest as there is nothing sacred about outgroup attitudes as an outcome measure. It has been suggested, that especially when intergroup conflict is still recent, changing personal attitudes toward the outgroup may not be as important, as practical, or as efficient as changing one's normative perceptions of the outgroup (e.g., Greenwald, Poehlman, Uhlmann, & Banaji, 2009; Paluck, 2009). In any case, assessing the effects of the two types of friendships on different outcome variables, especially ones that tap into reconciliation, like trust, forgiveness, and intentions for peaceful coexistence, would shed more light on what the contribution of direct and extended friendships is in improving intergroup relations in contexts like Cyprus.

In this study two potential mediators were tested as mechanisms that could explain the longitudinal effects of the two types of friendships. These were ingroup norms and intergroup anxiety. One could ask why only ingroup and not outgroup norms were tested in this study. The simplest answer to this question is that the main objective of this study was to compare the longitudinal effects of direct and extended friendships and their mediation and therefore it made more sense to balance the number of the mediators so that I have one mediator perceived and found to relate strongly to direct friendships, i.e., intergroup anxiety, and one mediator that was thought and found to relate strongly to extended friendships. Given this rationale, ingroup and outgroup norms could be used interchangeably as they were both conceptualised by Wright et al. (1997) to be mechanisms explaining the effects of extended friendships on prejudice. In fact, I did try out a model where outgroup norms replaced ingroup norms and this model yielded essentially the same results as the one presented here, i.e., Time 1 extended as well as direct friendships advanced positive outgroup attitudes at Time 3 via intergroup anxiety at Time 2, but only Time 1 direct friendships had an (marginal) effect on

Time 2 outgroup norms. Time 2 outgroup norms had a positive effect on Time 3 outgroup attitudes.

A surprising result in this study was that the direct or indirect ‘reverse’ paths from Time 1 attitudes to Time 3 friendships were not found to be significant. I attributed this null effect to the lack of opportunities for any form of contact in the Cypriot context thus suggesting that because individuals with positive evaluations of the outgroup do not have opportunities to meet the outgroup in their everyday lives, their openness to the outgroup did not end up translating into cross-group friendships. I propose that opportunities for contact can be tested as a moderator of the existence of ‘reverse’ paths from prejudice to contact. In contexts providing plenty of opportunities for contact, selection bias might be more likely to be observed than in those contexts where opportunities for contact are fewer. Even though this is merely a hypothesis a look into the studies that have taken up the directionality of the contact-prejudice relationship, showed that the ‘reverse’ path was consistently present in fairly mixed contexts like for example, a racially mixed university setting in Levin et al.’s (2003) study, and mixed schools in Binder et al.’s (2009) study. The only notable exception to the findings of these studies are the findings of the study by Feddes et al. (2009) who found no support for the ‘reverse’ path despite this study being conducted in a context providing plenty of opportunities for intergroup contact.

Methodological limitations

While in this study I tried to take into consideration the most recent methodological recommendations, there remain some sources of potential criticism on this front. I will discuss two of them in this section: a) using measured (manifest) instead of latent variables to measure direct and extended friendships, b) relying wholly on the cross-lagged analysis technique for

longitudinal data. With regards to the first point, even though I argued in favour of the use of latent as opposed to observed variables, my two predictor variables, direct and extended friendships, were treated as manifest variables. In reality the two types of intergroup friendships were measured with multiple items. For direct friendships two more items were included: 1) ‘how often do you visit your Turkish Cypriot friends?’ and 2) ‘how often do your friends visit you?’. Extended friendships were measured via three items. The two non-included items were: 1) ‘how many members of your immediate family?’ and 2) ‘how many of your colleagues, have outgroup friends?’.

I decided nevertheless to keep the single friendship items for three main reasons. The first pertained to the fact that the item I used to measure direct and extended friendships specifically referred to the amount of friendships. While the other two items tapping extended friendships also concerned amount of friendships, the two items additional items tapping direct friendships asked about number of visits from and to the outgroup friends. The latter two items do not refer to amount of friendships and so I thought that if I did end up using all three items for each scale this would render the two constructs less comparable. The second main reason for keeping the item tapping friendships via friends to measure extended friendships was to keep as close as possible to the definition of Wright et al. (1997) of extended friendships (i.e., having a friend who has an outgroup friend). The third reason is that the items I ended up retaining are most similar which enabled me to best compare the two types of friendships.

Concerning the second point, I intentionally employed the cross-lagged modeling technique for analysing these data, as it seemed the one most suitable to aid me in answering the research questions I was interested in. I should mention, however, that there are a number

of ways for longitudinal data to be analysed. The most noteworthy alternative analysis technique is Latent Growth Curve Modeling. Latent Growth Curve models (Meredith & Tisak, 1990) are suited to exploring individual differences in the degree and fashion in which change occurs (within individuals) over time. In Latent Growth Curve models, each individual's scores on a variable of interest can be expressed as a function of time (Maxwell & Cole, 2007). The idea here is that each person can potentially have a unique trajectory of change overtime which is in direct contrast to the underlying idea behind the cross-lagged models where it is assumed that the autoregressive coefficients are the same for all individuals (Christ & Wagner, 2013; Hertzog & Nesselroade, 2003).

To illustrate, using an example from intergroup contact, whereas a temporal change in prejudice may or may not be found at the aggregate level, the Latent Growth Curve models will be able to inform one whether individuals can be differentiated in terms of the degree to which they follow the overall trend or not. Dhont et al. (2011) for instance, found that there was no change in contact and prejudice over-time at the aggregate level but that at the individual level there was variation between individuals, i.e., some individuals showed an increase in their levels of contact over-time and some showed a decrease. They found that in those cases where there was an increase of the amount of contact over-time there was decrease in the levels of prejudice overtime.

In addition to the above, especially when change is expected to occur, Latent Growth Curve models can be used to compute complex multivariate change models (Ployhart & Vandenberg, 2010). Importantly, they can address the question of what explains the time trajectory (i.e., changes) of a variable. More specifically, through Latent Growth Curve models, one can investigate whether temporal changes in one variable can bring about

temporal changes in another variable, or whether exogenous predictor variables that do not vary in time (e.g., gender) can predict the change over-time in the outcome variable.

Latent Growth Curve Models undoubtedly provide contact researchers with an excellent tool to better understand the dynamic relationships between contact and prejudice overtime and also to look into specific individual or contextual variables that may be affecting these relationships. With regards to the current study, however, I deemed that since my interest was at the aggregate level and since change in the variables of interest was not expected to occur over time, the cross-lagged models were the most appropriate method of analysis.

Conclusion

In the introduction to this chapter I placed an emphasis on explaining why longitudinal studies more than cross-sectional studies form reliable tools for addressing the research questions contact theorists have been primarily interested in. Apart from longitudinal studies being methodologically more sound when it comes to testing the direction of causation (and thereby mediation), longitudinal studies can furthermore inform us on whether the contact effects are strong enough to last in time or not.

This longitudinal study aimed at advancing the knowledge derived from past longitudinal as well as cross-sectional studies with regards to the comparative effectiveness of direct and extended friendships in reducing prejudice, and with regards to differences in the mediation of these effects by intergroup anxiety (a mediator which was traditionally linked to direct friendships) and ingroup norms (a mediator initially proposed to explain the effects of extended friendships on prejudice reduction). The current study (Study 6) also investigated the ‘selection bias’ hypothesis, in other words, whether less prejudiced individuals end up having

more friendships (direct or extended) in the long run. The study was conducted in Cyprus, an extremely segregated context, amongst individuals with limited intergroup contact levels. These characteristics of the context were taken into account in forming the hypotheses for this study and also in interpreting its findings.

By utilizing a representative sample of the Greek Cypriot adult population and by employing a three-wave longitudinal design this study provided a thorough examination of the effects of intergroup contact on prejudice and their mediation. Study 6 provided strong support for the prejudice-reducing effects of both types of friendships. This finding is of particular importance for extended friendships since, unlike direct friendships, extended friendships were not consistently found to lead to a longitudinal effect on prejudice reduction by past research. Despite the effectiveness of extended friendships in yielding longitudinal effects, however, these effects were still not as strong as the effects of direct friendships. The superiority of direct friendships over other types of contact (including extended friendships) was never contested, and this finding is in line with findings of previous studies showing stronger effects for direct as opposed to extended friendships (e.g., Paolini et al., 2004; 2007).

Study 6 furthermore shed light on the question of how (i.e., by which processes) the prejudice-reduction effects of the two types of friendships occur. Albeit somewhat unexpected, the results spoke in favour of the indirect effects of direct but not extended friendships on attitudes via ingroup norms. On the other hand, intergroup anxiety stood out as a common mediator of the effects of direct, as well as extended, friendships on outgroup attitudes. What is also notable is that the effects of both types of friendships on attitudes were fully mediated by either both of the mediators in the case of direct friendships or by only one of the mediators (intergroup anxiety) in the case of extended friendships. Study 6 specifically

tested whether mediation was full or partial by adding the direct paths from Time 1 friendships to Time 3 outgroup attitudes and their ‘reverse’ paths to the bidirectional mediation model. The finding of full mediation provided strong support for one of the assertions I made while interpreting the findings of the experimental data in Chapter 3, which showed that whereas neither direct nor vicarious contact interventions led to a more long-term effect on outgroup attitudes what they were successful at doing in the long-term was reducing participants’ anxiety about future contact. Study 6 showed that direct and extended friendships (just like direct and vicarious contact) first reduced participants’ anxiety about intergroup contact, and reduced anxiety then led to a better evaluation of the outgroup.

No support was given to the ‘selection bias’ hypothesis by the results of this study. While outgroup attitudes did have a longitudinal effect on anxiety and ingroup norms, no direct or indirect effect occurred from outgroup attitudes to friendships of either kind over-time. Even though this is a result that was not in line with my hypotheses, the absence of ‘reverse’ paths reinforces the basic tenet of the contact hypothesis in terms of the directionality of the events (i.e., intergroup contact leading to prejudice reduction, as opposed to less prejudiced individuals having more contact). Even in a setting where one would, in fact, expect to observe selection bias, the results gave absolute support to the intergroup contact hypothesis, with regards to the effectiveness of both direct and extended friendships in reducing prejudice over-time. The positive effects of extended friendships, in particular, back up the importance of investing indirect forms of contact, particularly in contexts in which more direct forms of contact had been, or still are, unfeasible, unwanted, or simply disregarded.

Chapter 6: Summary and Conclusions

This thesis aimed to assess the effectiveness of different types of intergroup contact in promoting favourable intergroup relations in Cyprus. The study of intergroup contact in Cyprus is only in its early stages but the existing studies have found support particularly for direct contact and its effects on prejudice reduction (e.g., Lyras 2007; Tausch et al., 2010). Much less is known about the effectiveness of indirect types of contact in leading to positive intergroup outcomes (see Husnu & Crisp, 2010a, for an exception). There is therefore ample space for contribution especially with regards to the study of the effectiveness of indirect forms of contact in Cyprus. Indirect forms of contact have been proposed to be (at least temporary) alternatives of direct contact (see Turner et al., 2007c), a stepping stone to actual contact (Eller et al., 2012; Gomez et al., 2011), and a means of preparing individuals psychologically for potential face-to-face contact (see Crisp & Turner, 2009).

Utilizing samples from the Greek Cypriot community, the studies of this thesis set out to assess and compare the effects of direct and indirect types of intergroup contact in promoting more positive intergroup outcomes in Cyprus. These studies aimed to advance existing knowledge on intergroup contact more generally in some important ways. These advancements pertained to: a) the investigation of both the short- as well as the longer-term effects of contact interventions, b) the comparison of the effects of different types of contact interventions in terms of their breadth, their magnitude, and their longevity, c) the exploration of the ability of indirect contact to prepare individuals for future direct contact, d) the comparison of two types of naturally occurring contact, direct and extended friendships, in terms of the mediation of these effects on outgroup attitudes, and e) a test of the self-selection hypothesis of the relationship between contact and attitudes. I will now proceed to provide an

overview of the main findings of the six studies comprising this thesis, which I will discuss in relation to their contribution to the field of intergroup contact. In the second part of this chapter I will outline potential limitations of this piece of work as well as directions for future research. I will end with some ideas on practical implementations deriving from these studies and some concluding remarks.

Summary of key findings

a) The longevity of the effects of intergroup contact

The research on intergroup contact has placed a great emphasis on whether, when, and how, contact works but much less is known about whether its effects last over time. I deem this question as crucial particularly when intergroup contact interventions are being evaluated. The vast majority of past studies assessing the effects of contact interventions either in the laboratory (see all imagined and vicarious contact studies with the exception of Vezzali et al., 2012; 2012) or in the real world (e.g., Al Ramiah & Hewstone, 2012; Eller et al., 2011) focused only on the temporary effects of contact as measured immediately after the intervention. As for naturally occurring contact, longitudinal studies aiming to establish the temporal effects of contact on prejudice have only recently started to grow and the great majority deal with direct and not indirect contact.

Experiments 2-3 utilized a pretest-intervention-posttest *and* delayed posttest design to assess whether different types of contact had effects on outgroup attitudes, intergroup anxiety, and action tendencies that lasted over time (i.e., a week after contact). The results of these two experiments were consistent: they showed that intergroup contact (direct and vicarious) led to more positive outgroup attitudes at posttest, but that these effects disappeared a week after contact. In fact, a week after contact, outgroup attitudes regressed to their initial (pre-contact)

levels. While the effects of contact on outgroup attitudes did not last in time, Experiment 3 revealed that unlike outgroup attitudes which became less favourable a week after contact, attitudes towards the outgroup *member* (i.e., the Turkish Cypriot the participants interacted with), which were positive at posttest remained positive a week later. Furthermore, the results of Experiments 2 and 3 yielded a reduction in intergroup anxiety that lasted in time,⁷⁷ whereas Experiment 3 revealed that action tendencies became more positive and remained positive a week after contact, at least for direct contact.

In contrast to experimentally induced contact, naturally occurring good quality contact was found, in Study 6, to have a significant longitudinal indirect effect on outgroup attitudes. This was the case for both direct and extended friendships. The finding that extended friendships had an indirect temporal effect on attitudes (via intergroup anxiety), was of particular importance since the temporal effects of extended friendships on prejudice have not received adequate support to date. Extended friendships were also found to have a significant longitudinal direct effect on prejudice (in this case, attitudes), as has been shown to be the case in segregated contexts (see Christ et al., 2010) and amongst people with little prior contact (see Cameron et al., 2011).

Combining the results of the experimental studies and the longitudinal study, one can see that a one-off interaction with an outgroup member for individuals with scarce prior contact cannot permanently change their evaluation of the outgroup, but it can lead them to develop (lasting) positive attitudes towards a member of the outgroup. This, coupled with the other benefits yielded by the one-off interaction, namely long-term anxiety reduction and (for some individuals at least) positive action tendencies, may have an impact on how one thinks about

⁷⁷ In fact the effect of both direct and vicarious contact on intergroup anxiety was only found at the delayed posttest in Experiment 2. Vicarious contact exhibited roughly the same pattern in Experiment 3 too (i.e., its effects on anxiety became significant only at delayed posttest).

and acts towards the outgroup when exposed to it in the future. Thereafter, if and when individuals open up to the outgroup and manage to make outgroup friends, this will, as Study 6 showed, have a more permanent impact on their attitudes towards the whole outgroup.

b) The superiority of direct contact for improving intergroup relations

Previous researchers have dubbed indirect forms of contact as *alternatives* (see Turner et al., 2007c) to direct contact that can also be *sufficient* (see Eller et al., 2012) in bringing about positive intergroup outcomes. Even though it is not entirely clear what the terms ‘alternative’ and ‘sufficient’ mean, I reasoned that a comparison of direct and indirect types of contact based on the strength, the comprehensiveness, and the permanence of their effects on prejudice-related measures, would shed more light on these propositions. I performed this comparison both for experimentally induced contact (see Experiments 1-3), as well as for naturally occurring contact, i.e., friendships (see Study 6). In the same vein, I also tested (see Experiments 1-3) Wright et al.’s (1997) contentions, that extended as opposed to direct contact will safeguard group salience which is essential for the generalisation of the effects of contact from the outgroup to the outgroup as a whole (see Brown & Hewstone, 2005), while also not exposing the individual to a (potentially) stressful occurrence (i.e., a face-to-face interaction with an outgroup member).

Experiments 1-3 showed that, overall, direct contact had a larger breadth of effects in comparison to the other two types of contact (vicarious and imagined) as it reliably yielded pretest-posttest changes on all dependent variables: (more favourable) outgroup attitudes (Experiments 1-3), (reduced) intergroup anxiety (Experiment 2-3), and (more positive) action tendencies (Experiment 3); while vicarious contact only gave rise to effects on outgroup attitudes (Experiments 1-3), and intergroup anxiety (Experiments 2-3), but not action

tendencies; and imagined contact only gave rise to effects on intergroup anxiety and action tendencies, but not on outgroup attitudes. All the effects of direct contact apart from outgroup attitudes lasted in time, and they were also significantly different from the effects of a control condition (see Experiments 2 and 3). In terms of the magnitude of the three conditions' effects, however, the only significant difference occurred between direct and vicarious contact in Experiment 1 where direct contact yielded more positive attitudes than vicarious contact. This difference was not replicated in the rest of the experiments.

Wright et al.'s (1997) postulations were assessed in Experiments 1-3 but no support was found for them. Participants in the vicarious contact condition did not report higher group salience or lower state intergroup anxiety in comparison to the direct contact participants. One could argue here that in real-world contact, where individuals do not typically meet outgroupers in an environment that is as controlled or as safe and reassuring as the environment of a university lab⁷⁸, direct contact *may* in fact yield more anxiety. But to the extent that contact is used as an intervention, a positive intergroup encounter, according to these experiments, should not necessarily cause more anxiety to the individuals engaging in it than the individuals watching it. Also, the risk of group salience being too low for direct contact, which is what Wright et al.'s (1997) argument was about, was not a factor in these studies. Despite the high interpersonal focus of the intergroup interaction, those elements that make the two groups (Greek Cypriots and Turkish Cypriots) different (and hence salient), were, I would argue, hard to neglect during the interaction⁷⁹.

⁷⁸ One should also bear in mind that the experiments were conducted at a university in the south (in most cases that was the participants' own university) and that the experimenter was Greek Cypriot. This probably made the participants feel more at ease and that could explain the very low anxiety levels they reported having experienced.

⁷⁹ Participants had to communicate with the confederate in English – the experimenter also presented the confederate as a Turkish Cypriot who lives in the other side of the island.

The results of Study 6 were largely in line with the experimental findings, as they also indicated the superiority of direct contact (friendships) over indirect contact (extended friendships). More specifically, Study 6 showed that whereas both direct and extended friendships had a longitudinal effect on attitudes, this effect was larger for direct friendships than it was for extended friendships. In the same study, direct but not extended friendships had a temporal effect on ingroup norms.

The results of these studies therefore show that, compared to other types of contact, direct contact forms the golden route to prejudice reduction both as an intervention as well as in the form of naturally occurring contact. Despite contestations that direct contact may be more intimidating and therefore stressful than indirect contact (see Wright et al., 1997), Experiments 1-3 showed that direct contact did not raise more state intergroup anxiety than vicarious (or imagined) contact. Extended contact, and particularly extended friendships which were suggested to be primarily effective in segregated contexts, were indeed shown in Study 6 to be able to bring about positive and lasting prejudice-reducing outcomes in the segregated Cypriot context. But these effects were nevertheless weaker than the effects of direct friendships. One should therefore be cautious in claiming that extended friendships can be thought of as permanent and equivalent alternatives to direct friendships (even in segregated contexts).

c) Can indirect contact prepare individuals for direct contact?

The real test for vicarious and imagined contact pertained to their ability to influence those variables that were more related to preparing the individual for future contact, namely intergroup anxiety and action tendencies. More specifically, indirect types of contact should ideally render individuals interested in, as well as willing to, either seize the opportunity for

contact when it arises or even seek more contact themselves. Indirect types of contact should also be able to psychologically prepare individuals to benefit from contact by reducing their anxiety for future face-to-face interactions. As was suggested and found in the past, intergroup anxiety can poison a potentially positive interaction (Richeson & Shelton, 2003; Stephan & Stephan, 1985) and therefore entering an intimate intergroup interaction with less anxiety is understood to be crucial. Whether indirect contact can have an effect on intergroup anxiety and action tendencies was examined in Experiment 3. Furthermore, apart from being an outcome, intergroup anxiety can also mediate the way from contact to action tendencies such that indirect contact leads to lower anxiety and lower anxiety leads to more willingness for future contact. This hypothesis was tested in Experiment 5 for imagined contact.

Experiment 3 showed that both vicarious as well as imagined contact had a lasting effect on participants' anxiety levels. Apart from the fact that anxiety levels were found at delayed posttest to be lower in comparison to pretest for these two conditions, delayed intergroup anxiety was also significantly lower than anxiety reported by the control group at delayed posttest, and was as low as the anxiety reported by the direct contact participants. When it came to action tendencies, however, it was only imagined contact, and not vicarious contact, that led to (at least temporary) positive changes in this variable.

The finding that vicarious contact did not lead to any changes in action tendencies is an interesting one and it is indicative of a potential difference between these two types of indirect contact. A possible reason for the differential effects of these two types of contact on action tendencies is that imagined contact places the self in the role of the actor and not in the role of the observer in an intergroup encounter. Finding oneself in an intimate encounter with an outgroup member is probably more catalytic in assisting the individual to acquire what Crisp and

Husnu (2011) called a “behavioural script” (p. 277) based on which to make inferences on how he or she feels and acts at the prospect (or in the actual incidence) of a face-to-face intergroup interaction.

Further support for the capacity of imagined contact to yield positive behavioural outcomes was given by the results of Experiment 5, where imagined contact was found to have an indirect effect on action tendencies which was mediated (as expected) by intergroup anxiety as well as contact self-efficacy⁸⁰. This effect, however, was found only for an amended scenario of imagined contact, the ‘balanced similarity’ imagined contact scenario and not for the ‘standard’ positive imagined contact. More specifically, the results of this study showed that whereas imagined contact, regardless of the condition, yielded more positive attitudes in comparison to the control condition, of the two imagined contact conditions, only the ‘balanced similarity’ condition led to effects on (higher) self-efficacy and (lower) intergroup anxiety, and an indirect effect on (more positive) action tendencies in comparison to the control condition. Positive imagined contact on the other hand, was undifferentiated from the control group on the variables of self-efficacy, intergroup anxiety, and action tendencies.

These results cast doubts on the tenet of Crisp et al. (2008) that imagining an interaction that is explicitly positive (see West et al., 2010) and has an intergroup character (see Pagotto et al., 2012; Stathi et al., 2011) will be *adequate* in yielding positive intergroup outcomes. This is not the first study that contradicted the postulation of Crisp et al. A recent study by Kuchenbrandt et al. (2013) has also challenged the view that imagining a positive and intergroup interaction satisfies the necessary *and* sufficient conditions for imagined contact to be effective.

⁸⁰ The mediating effects of intergroup anxiety and contact self-efficacy will be discussed in more detail below.

Experiment 5 (and the study by Kuchenbrandt et al., 2013) suggest that in some cases (i.e., in certain contexts, for certain outgroups, or for certain individuals), imagining a positive interaction may not suffice to bring about positive intergroup outcomes. While one could perceive this to be a blow to the positive intergroup imagined contact scenario, I personally believe that this is not the case. In my view, imagining a positive interaction is already breaking down the mental barrier between the ingroup and the outgroup as well as the default belief that an interaction with the outgroup is going to be stressful or negative (see Mallet et al., 2008, for more on this). If imagined contact were to be used for intervention purposes then I think that imagining a positive interaction is a good place to start from since the positive intergroup contact scenario encompasses in my opinion the *necessary* conditions needed for positive outcomes to occur, which at times, however, may not be *sufficient*.

Having said that, I should also clarify that I do not believe that the way forward for imagined contact is for researchers to come up with a whole list of new conditions that they deem as necessary (when they are, in fact, facilitating), as happened with direct contact some decades ago (see Pettigrew, 1998). Given that each intergroup situation is different and each intergroup conflict has its own defining characteristics, I think that the imagined contact scenario can be adjusted accordingly so as to correspond to the particularities of the context or the individuals involved. In my studies, for example, what stood out was the overwhelming lack of contact the participants had with the outgroup. This lack of contact went hand in hand with participants' notable anxiety for (future) contact (see Experiments 1-3). Perhaps because of the lack of previous contact of my participants with the outgroup, a more elaborate imagined contact scenario was needed in order to yield more positive intergroup outcomes than a merely positive intergroup interaction. In the same way, in a context where intergroup

contact is more common and likely but is often negative, imagining a *positive* intergroup contact should suffice or might even be more effective in causing more positive outcomes than scenarios focusing on elements other than the positivity of the interaction.

d) Mediators of contact: intergroup anxiety, norms, and the promising role of contact self-efficacy

Even though different types of contact may work via different processes, past studies have shown that there are some mechanisms that can explain both direct and indirect contact's effects on prejudice. One of them is intergroup anxiety reduction. In line with this, Study 6 showed that the longitudinal effects of both direct and extended friendships on outgroup attitudes were mediated by intergroup anxiety (reduction). There was also some weak support for the mediating role of intergroup anxiety for the effect of ('balanced similarity') imagined contact on action tendencies (Experiment 5). I should also point out here that Experiments 2 and 3 showed that direct and indirect types of contact alike had an effect on anxiety. Interestingly in these two experiments, direct and indirect contact had a lasting effect on anxiety, but not on outgroup attitudes. Similarly, in Study 6, there was no direct effect from Time 1 friendships to Time 3 attitudes, but only an indirect effect via Time 2 anxiety. These results suggest that intergroup anxiety reduction is a first and crucial step that needs to be reached in order for contact to lead to a permanent change in one's evaluation of the outgroup (i.e., positive outgroup attitudes), at least in this setting.

Unlike intergroup anxiety reduction, the change towards more positive norms, was proposed (and found) to be more apt to explain the effects of extended rather than direct contact (e.g., De Tezanos-Pinto, 2010). This hypothesis was tested in Study 6, but no support was found for it. In fact the results pointed to the exact opposite direction. Ingroup norms only

mediated the effects of direct friendships on attitudes. Extended friendships had no influence on ingroup norms. I tried to explain these findings by saying that when ingroup norms are unclear, then one's own intergroup behaviour (i.e., direct friendships) may be more likely to influence one's norms. Another explanation, which cannot be based on the available data but can be checked though, is that in a context in which intergroup contact is certainly not the norm an ingroup member known to have intergroup contact may be more easily singled out as an exception to the rule (i.e., subtyped) by the observer of this friendship (rather than by the person having the friendship himself/ herself). As an effect, in contexts where intergroup contact (and more so friendships) is (are) not the norm, the behaviour of the ingroup individual who has an outgroup friend may not be perceived as normative by the observer of the friendship, who therefore ends up not changing his/her view on ingroup norms. Once again, this is a speculation which can only be addressed with a study testing ingroup norms as a mediator in both diverse and segregated contexts.

Finally, a relatively new mediator of the effects of contact was explored in Experiment 5. In this experiment I assessed the role of contact-related self-efficacy (see Mazziotta et al., 2010; Stathi et al., 2011) in mediating the effects of imagined contact on action tendencies. The role of self-efficacy as a mediator has not received much attention to date, and self-efficacy was never tested as a mediator of imagined (or direct) contact's effects. In fact, the construct of contact self-efficacy has not been a common criterion variable within intergroup contact research.⁸¹ It could, however, in my opinion, form a key variable capable of explaining the effects of contact particularly amongst individuals whose previous

⁸¹ It is the case, however, that previous studies included items that could qualify as contact self-efficacy like for example the study by Van Laar, Levin, Sinclair, and Sidanius (2005) who included a criterion variable they called "multicultural competence" (p. 333) which asked the participants how *competent* they felt about interacting with people from different groups.

unwillingness to approach the outgroup is less related to their negative view of the outgroup but more related to their anxiety about intergroup encounters and long-standing alienation from the outgroup.

e) Does contact reduce prejudice or does prejudice reduce contact?

The experimental nature of the first five studies and the longitudinal nature of the final study allowed for tests of directional hypotheses. While Experiments 1-3 and Experiment 5 provided strong support for the ability of contact to reduce prejudice (by e.g., leading to more favourable attitudes and reducing anxiety), in Study 6 I was also able to test whether prejudice can have an impact on contact too. My expectation was that I would find support both for the effect of contact on attitudes (forward path) as well as for the effect of attitudes on contact (reverse path). My reasoning for the reverse paths was that in a context like Cyprus, in which the number of people who have contact is very small and where contact needs to be sought after as there are few opportunities for it in everyday life, one's positive orientation towards the outgroup at Time 1 would directly or indirectly influence one's number of direct (and extended⁸²), friendships at Time 3. The results, however, yielded only a forward (indirect) path from friendships to attitudes, but no evidence for a direct or an indirect path extending from attitudes to either direct or extended friendships.

Interestingly enough, going back to previous studies addressing the 'direction of causation' question and through that the reverse (as well as the forward) paths between contact and prejudice, the reverse path was more likely to be significant in diverse contexts (see Binder et al., 2009; Levin et al., 2003, but for an exception see Feddes et al., 2009). This

⁸² My reasoning for believing that an effect of attitudes on extended friendships would be found was based on the idea that if individuals select their (ingroup) friends based on common values (see Byrne, 1971) and given that in contexts of conflict values regarding intergroup relations are rather prevalent, then the possibility should exist that individuals who are more open to the outgroup have friends who are more open to the outgroup and who are therefore likely to have outgroup friends.

led me to argue that opportunities for contact might moderate the likelihood of detecting reverse paths. I think one should, in principle, expect more tolerant people to be more open towards the outgroup and this should translate to individuals with less prejudice having more friends. But this might not be the case in contexts not providing adequate opportunities for contact because one's positive dispositions cannot easily translate into contact with the outgroup.

As far as the forward path from contact to prejudice is concerned, it could be hypothesized that this path would be even stronger in those contexts where opportunities for contact are scarce. In these contexts the effects of positive and good quality contact, like direct and extended friendships, should have guaranteed success in bringing about positive outcomes, particularly because managing to have good quality contact in an environment where the odds of having any type of contact are scarce is something which is commendable to begin with, but also likely to be very influential. This hypothesis derives support from the findings of Al Ramiah, Hewstone, Voci, Cairns, and Hughes (2011), showing that the effect of outgroup friendships on ingroup bias was moderated by the number of prior friendships, such that the smaller the number of prior friendships the bigger the effect of newly acquired friendships on reducing ingroup bias was. I should note here, that the magnitude of the effects of contact on attitudes (or prejudice in general) in mixed (diverse) vs. segregated contexts was never tested in the past.

Limitations of the present studies and directions for future research

Through my studies I tried to fill both theoretical and methodological gaps in the intergroup contact literature. There were, however, some limitations and omissions which I identified and discussed in greater detail in the discussion of each of the empirical chapters

(i.e., Chapters 3 – 5). In this section I focus on some further limitations which apply to all or the vast majority of the studies in this thesis. Alongside these additional limitations, I will propose avenues for future research.

Operationalisation of vicarious contact and its comparison with imagined contact

Four out of six studies reported here focused on the comparison of the effectiveness of different types of intergroup contact including the comparison of direct and indirect (vicarious and imagined) contact as well as the comparison of the two types of indirect contact to each other. Out of these four studies though, only one (Experiment 3) included imagined contact. That means that the results obtained by Experiment 3 were not replicated. Experiment 3, nevertheless, indicated some similarities and differences between imagined contact and each of the other two types of contact (direct and vicarious) which can be useful points of departure for further studies.

I would, in line with Crisp and Turner (2013), support a further comparison of these types of contact, particularly the two indirect types of contact that can be employed as interventions even in the absence of an outgroup. For future studies, however, I would recommend utilizing a different operationalisation of vicarious contact. Even though the operationalisation of vicarious contact used in Experiments 1-3 (i.e., observing a friend interacting with an outgroup) worked out well when it came to testing Wright et al.'s (1997) postulations, I think few would disagree that for vicarious contact to be used as an actual intervention, watching a friend interacting with an outgroup would not be the most feasible or even the most appropriate way to go about it⁸³. For a further exploration of vicarious

⁸³ I brought up the appropriateness of the operationalisation because, as I have mentioned in the discussion of Chapter 3, observing one's friend interacting with such intimacy with a stranger (who was also an outgroup) might not have been seen with a positive eye by all individuals assigned to the vicarious contact condition. It could have caused feelings of discomfort or jealousy. A very interesting and useful comparison would be

contact's capacities as an intervention, I would use operationalisations of vicarious contact that are more fit to implement in real-life, such as observing a clip portraying a positive intergroup interaction (e.g., Mazziotta et al., 2011), reading a story (e.g., Cameron & Rutland, 2006), or watching extracts from existing documentaries or films (e.g., Cole et al., 2003).

The benefits and challenges of a delayed posttest

I argued earlier that the inclusion of a delayed posttest in Experiments 2 and 3 allowed for the test of the longevity of the intervention's effects as well as for detecting any contact effects that had a delayed onset. In the same vein, I believe that Experiment 4 and especially Experiment 5 would have benefitted from a delayed measurement point. As I mentioned earlier, there were some changes taking place between immediate and delayed posttest in Experiments 2 and 3. Outgroup attitudes, for example, were found in the vast majority of the cases to be more positive at the immediate posttest than at pretest, but in all cases they regressed back to pretest levels at the delayed posttest. Furthermore, for vicarious contact, intergroup anxiety did not significantly reduce at posttest, but for some reason its effects became significant a week later (delayed posttest).

While I was able to register these changes with the help of a delayed posttest, what I was not able to do was to find out what caused these changes to occur. In order to unravel the reasons for these changes I could have added measures tapping the interaction (or rather the memory of it) at a later stage (i.e., at the delayed posttest). Participants' (positive) feelings about the interaction could have changed, for example. Participants might have also, in

between a version of vicarious contact where the individual observes an ingroup stranger interacting with an outgroup stranger and a version of vicarious contact where the individual observes one's friend interacting with the outgroup, to see how these two versions of vicarious contact differ particularly with regard to the intrapersonal processes they elicit. I would think that while on the one hand observing a friend interacting positively with an outgrouper may cause negative feelings, it could, on the other hand, be beneficial as an ingrouper who is known to the individuals forms a reliable source of information and is less likely to be seen as a "black sheep" or an exception to the rule, in comparison to unknown ingroupers.

retrospect, changed their cognitive representation of the interaction: e.g., they might have looked back to the interaction as an interpersonal rather than an intergroup event, which would help explain why generalisation of positive attitudes from the outgroup (confederate) to the outgroup as a whole only took place at posttest (i.e., when participants were reporting that the two groups were moderately salient at contact).

Furthermore, asking questions about the interaction at a later stage can inform one about how vividly participants remember the interaction. Husnu and Crisp (2010b) contested that an imagined contact scenario that manages to stay strong in memory will be more influential in affecting individuals' behavioural intentions. A strong memory of the contact event, they argue, will provide participants with an available script to which they can resort when having to make judgements about their intergroup behaviours. Also, being able to remember a positive encounter may buffer against prospective stressful intergroup encounters (see e.g., Page-Gould, Mendes, & Major, 2010 who found that prior contact can help individuals recover from a stressful intergroup evaluation). The capacity of contact to form a strong memory imprint could therefore become a criterion variable which would be of particular usefulness in evaluating contact interventions.

Individual characteristics as moderators of different contact interventions

Whereas the studies comprising this thesis were very thorough in comparing different types of contact (both interventions and naturally occurring contact), we have little knowledge of some potential moderators of (different types of) contact in this context. There is a growing literature showing that contact may be even more beneficial amongst high-prejudiced people, i.e., individuals who are high than low on the Social Dominance Orientation scale (e.g., Hodson, 2008), individuals who score higher on Right Wing Authoritarianism (e.g., Dhont &

van Hiel, 2009), individuals with high need for closure (Dhont, Roets, & van Hiel, 2011; Study 2), individuals who identify more strongly with their ingroup identity (Hodson, Harry, & Mitchell, 2009), and individuals with more negative initial attitudes (e.g., Munniksma et al., 2013). This was shown to be the case both for direct contact (e.g., Hodson, 2008) as well as for extended contact (e.g., Dhont et al., 2011; Munniksma et al., 2013). No such evidence has been found for imagined contact so far. Husnu and Crisp's (2010b) study, which assessed the moderating effect of previous (good quality) direct contact on imagined contact's effects on behavioural intentions, however, showed that people with prior contact were more likely to report greater intentions to engage in future-contact. If we consider that people with more high quality contact are also less prejudiced, then Husnu and Crisp's (2010b) results suggest that imagined contact is actually more beneficial amongst low-prejudiced individuals.

Hodson et al. (2013) remarked that intergroup interactions may only be beneficial for high prejudiced people if these individuals do not feel coerced to change their attitudes towards the outgroup, i.e., when contact is self-initiated and when exposure to the outgroup is passive rather than orchestrated. This means that contact-interventions (like imagined contact) as opposed to naturally occurring contact, may backfire for high-prejudiced people if they feel they are being forced to change their views on the outgroup.

The hypothesis outlined above is an interesting hypothesis that has barely been explored. Maoz's (2003) study forms one of the few contact-intervention studies that addressed the question of whether a structured contact intervention would be beneficial for more prejudiced individuals. For this purpose, Maoz (2003) divided the Jewish-Israeli young participants who formed the ingroup of interest of her study into two groups based on their political affiliations with regards to the Israeli-Palestinian conflict. The two groups were the

“hawks” and the “doves”. “Hawks” were those individuals who opposed compromise and concessions in the conflict, were more inclined to combating the other side, and felt more threatened by it. “Doves” were those individuals who were more in favour of cooperation, negotiation, and problem-solving, and felt less threatened by the outgroup. The political affiliations of the participants were measured at pretest before a two-day structured dialogue between Israeli and Palestinian youth. The results showed that even though the “doves” were more positively inclined to the forthcoming intervention (as recorded at pretest) and stated that they were more satisfied with the encounter (as recorded at posttest) in comparison to the “hawks”, pre-to-post test changes in attitudes towards Palestinians were only found for the “hawks” and not the “doves”.

It should be noted here that the dialogue over the two days of the intervention in Maoz’s (2003) study allowed/ encouraged participants to talk about their respective cultures, their own experiences of the conflict, and their own national identities and political views. Other studies with ‘hawkish’ individuals have shown that these individuals tend to question sources of information that do not correspond to their views (Bar-Tal, Raviv, & Freund, 1994) and to discard messages that contradict their initial views (Maoz, 1999). To me these findings suggest that interventions will be beneficial for high-prejudiced people, if these individuals do not feel coerced or in some way threatened by the contact-intervention⁸⁴.

Before concluding this point, I also want to point out the benefits of a pretest-posttest design in assessing interventions when individual characteristics that may be moderating the effects of contact are evaluated. In my opinion, individual characteristics including: attitudes,

⁸⁴ An example of a contact intervention that threatened individuals in my studies was the ‘high similarity’ imagined contact condition in Experiment 4. As no moderators were tested in this experiment I cannot with certainty say whether prejudiced individuals were even more threatened by it, but this possibility is rather high given results of previous studies (e.g., Crisp et al., 2006).

socio-ideological beliefs (e.g., Right Wing Authoritarianism, Social Dominance Orientation), and strength of identification with ingroup identity *need* to be measured at pretest if they are to be used as moderators of the effects of contact (intervention) on prejudice. This is not necessarily common practice in experimental studies as possible moderators are often measured at posttest (see e.g., Stathi & Crisp, 2008). The reason for including moderators at pretest is that these individual characteristics are likely to be influenced by contact (see Dhont, Van Hiel, & Hewstone, 2013, for contact's impact on Social Dominance Orientation; Schmid, Hewstone, Tausch, Cairns, & Hughes, 2009, on the effect of contact on social identity complexity), in which case measuring them at posttest would not serve the purpose of testing them as moderators of the effect of contact.

Participants

This thesis focused entirely on Greek Cypriots mostly for practical reasons as this was the group I had (easier) access to. I cannot therefore generalise the findings of these studies to both communities living in Cyprus. One can recall from the introductory chapter on Cyprus that Greek Cypriots are the majority group in the country. It is by now well established (see Tropp & Pettigrew, 2005) that contact is more effective with majority rather than minority groups. This means that what was found to be the case in these studies with regards to the effectiveness of contact on attitudes (among other measures) for Greek Cypriots may not be replicated, at least not to the same extent, with Turkish Cypriots.

While research encompassing both groups should really be given more attention in Cyprus, I would not entirely agree that intergroup relationships in Cyprus are mostly defined by a majority/minority asymmetry, at least not for all age groups. To be more exact, research has shown an asymmetry between age-groups within the two communities in Cyprus and

differences between communities within age groups. To start with the inter-communal differences, Sitas et al. (2007) found that while the majority (62%) of *older* Greek Cypriots (i.e., people who have experienced the bi-communal conflicts and military interventions) reported contact between the two communities to have been positive prior to the division, the majority of Turkish Cypriots (55%) viewed prior bi-communal contact as rather neutral. Sitas et al. (2007) also found that of those individuals in their overall sample who adopted the view that reconciliation is possible, the majority were older Greek Cypriots. As for age differences within each community, age in the Turkish Cypriot community was found to be a determinant of vote in the reunification referendum, with young Turkish Cypriots being more likely to vote 'yes' than older Turkish Cypriots (Lordos, 2009). Interestingly, the greatest and most overwhelming rejection of the reunification plan in 2004 came from young Greek Cypriots (Jakobsson-Hatay, 2004; Lordos, Kaymak, & Tocci, 2008). Also Zembylas et al. (2011) found that amongst Greek Cypriot educators, the younger ones were less inclined to endorse the goal set by the Republic of Cyprus' (Greek Cypriot) Ministry of Education for peaceful coexistence and cooperation amongst communities in Cyprus.

These findings suggest that whereas the classic majority/minority dynamics can be seen in the older generation of Cypriots, perhaps because they experienced living together in a majority-minority setting, the same dynamics have not been reproduced in the young generation of the two communities who never got to live together. What can be also made out from the above findings is that the most intransigent segment of the whole Cypriot population is actually Greek Cypriot youth who, I should note, comprised almost exclusively my sample in these studies. I would also add, based on the above findings, that ethnicity (Greek Cypriot vs. Turkish Cypriot) but also age group (young vs. old) would both form potential moderators

of the effects of contact on prejudice within the Cypriot context and future research should take these two dimensions into account.

Practical implications

Study 6, which utilized a representative sample of the whole Greek Cypriot population, showed that direct as well as extended friendships can have longitudinal effects on attitudes. While this is good news, what is not good news is that the number of people reporting having direct, or extended, outgroup friendships, in this study was small (85.3% of the respondents reported having no direct friendships, and 60.5% of them reported having no friend with outgroup friends). In the same study, amongst young Greek Cypriots (i.e., individuals between 18 – 35 years), direct and extended friendships were even fewer (92.3% of the respondents reported having no direct friendships and 76.5% of the respondents reported no extended friendships). Mere contact (and not friendships) was also found to be minimal amongst young Greek Cypriots (see Experiments 3 and 5).

In the four experimental studies where the effects of various contact interventions were tested either against pretest scores (Experiment 1), or against a control group (Experiment 5), or both (Experiments 2 and 3), results were positive for contact. Even though participants reported rather high levels of intergroup anxiety before the interaction (see Experiments 1-3), contact regardless of its type (direct, vicarious, imagined) was not perceived to be anxiety-provoking, and it yielded either temporary or lasting effects on a set of dependent variables. One could argue therefore that young Greek Cypriots may be ready to reap the benefits of a good quality intergroup interaction and that they can benefit from direct and indirect contact interventions.

As I discussed above, however, interventions as opposed to naturally occurring contact can backfire especially amongst high-prejudiced individuals, if they find themselves being pressurized to change their views on the outgroup or if their identity is threatened. As one can tell from Experiments 4 and 5, contact interventions, and imagined contact in particular, come with the unique benefit of being customized to fit the particularities of the context or the individual. Of course, one would expect that contact meeting Allport's (1954) conditions would be, in principle, effective (and this counts for indirect types of contact too [see Gomez & Huici, 2008; Pagotto et al., 2012]), but contact interventions can accommodate other conditions that may be necessary in certain contexts or amongst certain individuals.

Yet, the big question that lingers is how contact interventions leave the lab and enter the real-world. An even greater question in heavily politicized contexts like Cyprus is whether contact interventions *can* leave the lab to enter the real-world. In Cyprus, intergroup contact (per se) has been an object of great controversy. This extract of the description of an imagined contact encounter of one of the participants may help me illustrate my point. This individual was assigned to a condition where participants were asked to imagine having an interaction with a Turkish Cypriot stranger through which they find out some interesting things about her (the Turkish Cypriot stranger). The participant said:

“She told me with hesitation that she is Turkish Cypriot. In the beginning I felt rather awkward but I feigned a positive reaction – I did not want to insult her. After a while the discussion evolved around the Cypriot problem. We exchanged opinions. She told me that her wish is for Greek and Turkish Cypriots to develop peaceful relations. She told me that her parents go to

various demonstrations in the occupied areas [she means the northern part] and carry the Cypriot flag. On one hand, I felt good with what she was saying, and I was in agreement, but on the other I was experiencing a very strange emotion. I was thinking that if anyone found out I was hanging out with a Turkish Cypriot they would call me names and they would classify me as a *traitor*. I did not, under any circumstances, want to find myself in such an uncomfortable situation” (emphasis added)

Whereas not everyone who has intergroup contact in Cyprus is labeled as a traitor, a prevalent feeling amongst Greek Cypriots is that unless a solution to the political problem (by which they mean justice for Greek Cypriots) is found, there is no room for contact.

Because intergroup contact is so emotionally loaded, any (rare) public policies favouring bi-communal dialogue have so far failed to be implemented. Zembylas et al. (2011) examined the reactions (difficulties and emotions) of Greek Cypriot teachers to a governmental initiative to set the promotion of a “*culture of peaceful coexistence*” between Greek Cypriots and Turkish Cypriots, as its central educational objective for the year 2008-2009 (Ministry of Education and Culture, 2008, as cited by Zembylas et al., 2011, p. 332). The initiative emphasized that education should highlight the elements that unite and characterize Greek Cypriots and Turkish Cypriots as one people, and that towards that end teachers were encouraged to “*get closer and become acquainted with the cultural expression of the two communities, so that they can transfer it to the students*” (Ministry of Education and Cultural 2008 as cited by Zembylas et al., 2011, p. 332). There was also a direction for the values of peaceful coexistence to be diffused in all aspects of school life and exchange of visits to and

from Turkish Cypriot schools came up as an idea. This suggestion was heavily criticized by the main trade union of teachers who issued a formal statement expressing their “*strong disagreement regarding the suggestion for visit-exchanges between Greek Cypriots and Turkish Cypriots*” (Zembylas et al., p. 332).

Zembylas et al. (2011) asked participants questions about their perceptions and emotions towards reconciliation and their perceptions toward the educational objective. In short the result showed that whereas teachers appreciated the importance of cultivating peaceful coexistence, they reported lacking the readiness and willingness to implement the new objective, while they also had their reservations with regards to its feasibility. Importantly (but not surprisingly), the younger teachers (aged 36 and under; born after the division of the island) were significantly less positive towards the new objective in comparison to the older cohort.

If there is one place where particularly indirect forms of contact can be implemented in the real-world, it is in schools (see e.g., Cameron & Rutland, 2006; Cameron, Rutland, Turner, Holman-Nicolas, & Powell, 2011; Liebkind & McAlister, 1999; Vezzali et al., 2012). In Cyprus’ case this has proven to be difficult mostly because teachers themselves (especially the young ones) do not feel ready to endorse these pro-contact policies.

This is, as one might appreciate, a rather complicated issue that cannot be addressed in a few lines at this point. I would, however, suggest that future research on intergroup contact in Cyprus expands in two important ways. The first way is to check for the capacity of contact (either naturally occurring contact or contact interventions) with outgroups other than Turkish Cypriots (e.g., economic immigrants) to yield secondary transfer effects (see Pettigrew, 2009, and Lolliot et al., 2013, for a review on the generalised effects of intergroup contact). If

contact with other stigmatized groups (e.g., economic immigrants) can have (extended) effects on individuals' attitudes towards Turkish Cypriots (as a secondary outgroup) then there might not be any need to touch upon the sensitive issue of bi-communal contact for positive effects to occur. The second way is to explore possibilities of creating interventions based on allegorical scenarios where no specific outgroup will have to be the target. An approximation of what I am referring to here is the teaching exercise developed by Hillman and Martin (2002) to tackle prejudice against homosexuals. Through this exercise one is encouraged to imagine the life on an alien planet where one gets to experience situational constraints akin to the ones homosexuals face in real life. The effects of this exercise's effectiveness on participants' anti-homosexual attitudes were tested by Hodson, Choma, and Costello (2009) who found that participants who were exposed to the teaching exercise in comparison to control participants reported more positive attitudes towards homosexuals both immediately after the exercise and a week later.

Concluding remarks

Both naturally occurring contact (direct and extended friendships) as well as contact interventions (direct, vicarious, and imagined contact) among (predominantly young) Greek Cypriots were found to have positive effects on a number of dependent variables targeting the outgroup (Turkish Cypriots). This is an important finding given that Cyprus is a context of marked segregation where contact predominantly amongst youth has been minimal and the idea of it generates feelings of anxiety.

In terms of its contribution to the intergroup contact literature, this thesis heeded the calls for a comparison of both the short-term and the long-term effectiveness of different types

of contact in yielding more positive intergroup outcomes, it explored further the capacity of imagined contact to yield more positive attitudes but most importantly to prepare for future contact, and it assessed and compared the longitudinal effects of naturally occurring contact (direct friendships and extended friendships) and the ways via which they bring about these effects.

The results showed that direct contact both as an intervention (Experiments 1-3), and when naturally occurring in the form of direct friendships (Study 6) can bring about effects that are more consistent (see Experiments 1-3), more lasting (see Experiment 3), and more profound (see Study 6). Notwithstanding the ability of direct contact to promote stronger, more comprehensive, and more permanent change in prejudice-related outcomes, indirect forms of contact (extended friendships, vicarious contact, and imagined contact) were also found to have some important effects.

Extended friendships, like direct friendships, were found to have a longitudinal effect on more positive attitudes towards Turkish Cypriots. These effects were indirect and they were mediated by intergroup anxiety. Vicarious and imagined contact were found to have significant and lasting effects on intergroup anxiety (Experiment 3), and imagined contact was found to have marginal effects on more positive action tendencies (Experiment 3). An amendment to the scenario of imagined contact (Experiment 5), furthermore, led to imagined contact having significant effects on a new variable (contact self-efficacy) via which it also yielded significantly more positive action tendencies in comparison to a no-contact control group.

Continued research on the effects of intergroup contact is needed to, amongst other things, inform and guide policies. If and when pro-contact policies cannot be implemented (as

in the case of Cyprus), however, then contact researchers interested in implementing contact in the real-world should come up with more innovative approaches to get around problems of this nature. I suggested that in the case of Cyprus, further exploration of the secondary effects of intergroup contact and of contact interventions not targeting a specific outgroup could lead to interventions with greater likelihood of actual implementation.

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Appendix 1
Closeness Induction Task (adapted from Sedikides et al. 1998)

List I (1 minute)

1. What is your first name?
2. How old are you?
3. Where in Cyprus do you live?
4. What do you study?
5. What year are you at University?
6. What is your favourite class, and why?

List II (3 minutes)

1. What are your hobbies?
2. What would you like to do after graduating from University?
3. What is something you have always wanted to do?
4. If you could travel anywhere in the world, where would you go and why?
5. What is one thing happening in your life that makes you stressed out?
6. What is one habit that you would like to break?

List III (5 minutes)

1. If you could have one wish granted, what would that be?
2. Is it difficult or easy for you to meet people? Why?
3. What is one of your biggest fears?
4. What is your most frightening memory from your childhood?
5. What is your happiest early childhood memory?
6. What is one recent accomplishment that you are proud of?
7. What things make you very happy?

Appendix 2

Five Factor Inventory (adapted from Costa & McCrae, 1985)

Instructions: Here are a number of characteristics that may or may not apply to you. For example, do you agree that you are someone who likes to spend time with others? Please circle a number for each statement to indicate the extent to which you agree or disagree with that statement.

I see myself as someone who...:

	Strongly Disagree	Disagree	Neither agree nor disagree	Agree	Strongly Agree
A1. Is helpful and unselfish with others	1	2	3	4	5
O1. Has few artistic interests	1	2	3	4	5
A2. Has a forgiving nature	1	2	3	4	5
E1. Is full of energy	1	2	3	4	5
A3. Likes to cooperate with others	1	2	3	4	5
A4. Is sometimes rude to others	1	2	3	4	5
E2. Is outgoing, sociable	1	2	3	4	5
O2. Is curious about many different things	1	2	3	4	5
O3. Values artistic aesthetic experiences	1	2	3	4	5
A5. Starts quarrels with others	1	2	3	4	5
E3. Is talkative	1	2	3	4	5
O4. Is ingenious, a deep thinker	1	2	3	4	5
A6. Is generally trusting	1	2	3	4	5

A7. Is considerate and kind to almost everyone	1	2	3	4	5
E4. Has an assertive personality	1	2	3	4	5
O5. Is original, comes up with new ideas	1	2	3	4	5
E5. Is sometimes shy, inhibited	1	2	3	4	5
O6. Is sophisticated in art, music, or literature	1	2	3	4	5
A8. Tends to find fault with others	1	2	3	4	5
O7. Likes to reflect, play with ideas	1	2	3	4	5
O8. Has an active imagination	1	2	3	4	5
E6. Generates a lot of enthusiasm	1	2	3	4	5
O9. Is inventive	1	2	3	4	5
A9. Can be cold and aloof	1	2	3	4	5
E7. Is reserved	1	2	3	4	5
E8. Tends to be quiet	1	2	3	4	5
O10. Prefers work that is routine	1	2	3	4	5